

THE PSYCHOLOGICAL REVIEW.

A STUDY OF GEOMETRICAL ILLUSIONS.

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With the single exception of Brentano's¹ unsuccessful attempt to explain the Müller-Lyer illusion by the general fact that acute angles are overestimated and obtuse angles are underestimated, no one has essayed to establish any direct relation between the illusions of linear distances and those in which there is false judgment of the angles. The so-called angle illusions have always been referred to the Poggendorff figure as the simplest case of the illusion. Since 1861, when Hering² first explained the Poggendorff illusion as due to the false estimation of the angles, there has been, in spite of disagreements as to the ultimate explanation of the false estimation, a universal acceptance of the statement that the angles are misjudged. It is the aim of this paper to present certain facts that seem to show that the false estimation of the angles in the Poggendorff figure is merely a secondary effect, not always present, and in no case the source of the illusion. The illusion is rather to be explained as due to the wrong estimation of certain linear distances, and may be reduced in the last analysis to the type of illusion found in the Müller-Lyer figure.

Before taking up the discussion of the Poggendorff illusion it will be necessary to point out certain facts in regard to the Müller-Lyer figure. The overestimation and underestimation characteristic of this figure are very much more comprehensive processes

¹Zeitsch. für Psych. und Phys., III., 349.

²Beiträge zur Phys., p. 384.

than they are ordinarily recognized to be. The attention of the observer has always been concentrated on the lines included between the obliques. In developing his theory of boundaries Lipps evidently sees that the concentration of the whole attention on a line within certain boundaries is a limitation which calls for some justification. He raises the question: How does it come that a line which bounds the figure within does not at the same time act as the boundary of the space without? The very important suggestion contained in this question is, however, entirely lost in the easy assumption with which Lipps dismisses the difficulty that he has raised. He assumes that when a line is in a position such that it may be regarded as the seat of the two non-contradictory functions which are relatively independent, one of these functions will appear, while the other will be swallowed up in the general perceptual process. Such a disposition of the matter does not find any justification in the empirical facts. Fig. 1 is made up of the two figures of the Müller-Lyer illusion so placed that the ends of the horizontal lines are equally distant from the short vertical line placed between them. It is evident that overestimation within the figure is accompanied by underestimation of the space outside of the figure, and, conversely, underestimation within the figure is accompanied by overestimation of the neighboring space. It may be objected that the presence of the short vertical line between the extremities of the horizontals gives us, in effect, two new Müller-Lyer figures of empty space in which the oblique lines will, of course, be directed in exactly the opposite direction to that in which they extend when regarded as parts of the original figures. The answer to this objection is that if no vertical line is used, but the subject is required to locate the point which is apparently half-way between the extremities of the horizontal lines, he will make an error indicating the presence of the illusion in its full intensity. This will be made somewhat clearer by Fig. 2. In this figure the obliques are drawn in such a way that with respect to the line as a whole they produce no illusion. The partial effects of the oblique lines are, however, by no means lost. If some neutral point of reference is marked so that direct comparison is possible.

¹Raumaesth. und geom. Täuschungen, III.

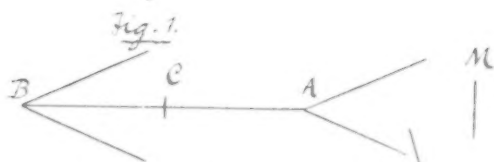
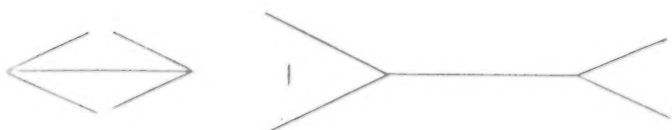


Fig. 2.

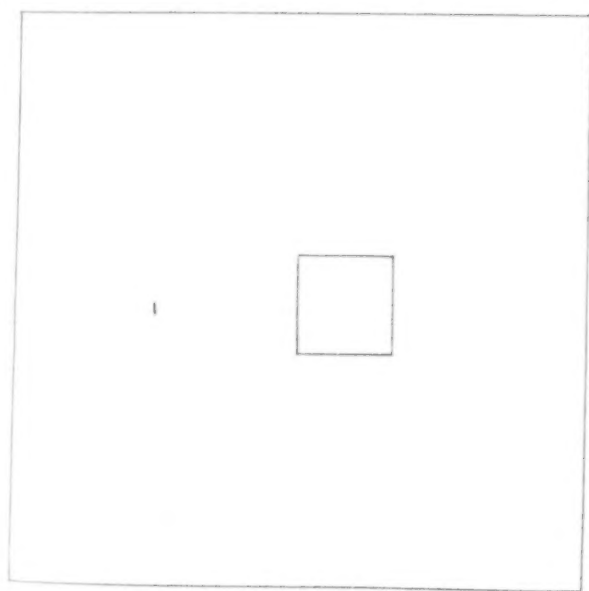
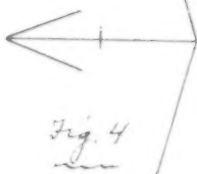
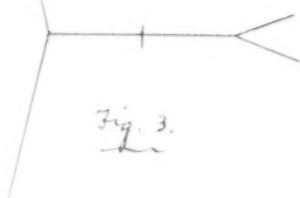


Fig. 5.

it can be seen that the partial effects are present in undiminished intensity. Thus in the figure the horizontal line as a whole suffers no illusion whatever. The middle point is marked, and it appears at once that in one-half of the line there is underestimation and in the other half there is an equal degree of overestimation. Furthermore, the position of the figure as a whole with respect to the short vertical lines, which are placed at equal distances from the extremities of the horizontal line, indicates that the influence of the oblique lines on the surrounding space is undiminished.

Other similar facts are illustrated by Figs. 3 and 4. In these figures the influences at the extremities of each of the horizontals are alike in kind, but unequal in degree. The result is, again, a shifting of the middle point. The illusion announced by Professor Baldwin in 1895¹ also belongs here. The illusion is in brief as follows: If two figures of unequal size, as two squares or two circles, are brought near to each other in the field of vision, the point half-way between them will be attracted towards the larger figure. The similarity between Fig. 3 and Professor Baldwin's circle figure is apparent at once. But the illusion appears when squares are used instead of circles. That even in this case the illusion belongs under the principle here developed, rather than under any principle of size-contrast, will be apparent from Fig. 5. Here the large square contains the small one, and yet the illusion is in kind and degree exactly like that described by Professor Baldwin.

All of these facts go to show that the processes of underestimation and overestimation within the figure are accompanied by far-reaching effects outside of the figures. In fact, overestimation and underestimation are wholly inadequate terms with which to describe the processes taking place. To say that points are shifted in their spatial relations with reference to all the points in the field of vision would be much nearer to the whole truth. Such shifting becomes apparent only when neutral points of reference are present in the field of vision, or when direct comparison with points which are shifted in the opposite

¹ PSYCHOLOGICAL REVIEW, II., 244.

direction becomes possible. When referred to this general principle it becomes evident that the phenomena of overestimation and underestimation are only occasional manifestations, under favorable conditions, of processes that are operative but undetected in every field of vision. In most cases the tendencies to false estimation of one sort or the other are lost in the complexity of the conditions or through the absence of points of comparison. One case in which such tendencies are present but unobserved is of such importance for our later discussion that we may call attention to it at this time. If an oblique line ends in a horizontal line which extends for an indefinite distance on each side of the point of meeting there will be no apparent illusion. If, however, the attempt is made to mark off in the horizontal line equal distances on each side of the point of intersection with the oblique line, it will be found that distances on the acute-angle side are underestimated and distances on the obtuse-angle side are overestimated. Or if the point of intersection is taken as the point of greatest importance, the illusion will take the form of the shifting of that point towards the extremity of the horizontal line which is on the acute-angle side.

Having thus generalized the concepts overestimation and underestimation, we turn to a consideration of the Poggendorff figure, and a criticism of the theory which regards it as the simplest case of the angle illusions. First of all, we have to consider certain negative evidences which show that the illusion is not due to a false estimation of the angles. Such negative evidence is at hand in the now generally known facts. The illusion disappears when the figure is so placed that the intercepted line is horizontal or vertical. If the illusion were due to wrong estimation of the angles it is not easy to see how these particular positions of the figure could operate to destroy the illusion. No general statement to the effect that an acute angle, one of the sides of which is vertical or horizontal, is not subject to false estimation will explain away this difficulty for the angle theory, for if the intercepting parallels, instead of the intercepted line, are brought into the vertical or horizontal position, the illusion appears in its full intensity. The negative evidence

presented in these four positions of the figure is strengthened by that given in Figs. 6, 7 and 8. In Fig. 6 the acute angles are present, but the illusion does not appear in any position of the figure. In Fig. 7 the parts of the intercepting parallels which lie between the points of interception are present, and with these the obtuse angles. The illusion is strengthened so much that it cannot be made to disappear in any position of the figure. The same is true of the case illustrated by Fig. 8, in which only a small part of each of the intercepting parallels is present. With regard to the angles, then, we must conclude that the acute angles, instead of being essential to the illusion, seem rather to weaken it, for the strongest form of the illusion appears when these angles are omitted.

Further negative evidence appears in Figs. 9 and 10. In Fig. 9 the upper and lower halves show the Poggendorff illusion in opposite directions. If the angles were misjudged, the interrupted lines should seem to diverge on the left and to converge on the right. As a matter of fact, it will be seen that all the oblique lines appear to be parallel with each other; this statement applying, of course, to the two interrupted lines when compared with each other. Furthermore, since the uninterrupted oblique lines form the same acute and obtuse angles with the intercepting parallels as do the intercepted lines, it is possible to make a direct comparison between the angles under discussion in cases in which the illusion is present and in cases in which it is absent. No inequality will be observable. On the other hand, it will be noted that the apparent width of the spaces between the oblique lines is not the same when the right and left sides of the figure are compared. Thus, the interval between the upper oblique and the interrupted line seems wider on the left than it does on the right. The space below the intercepted line seems broader on the right than on the left. The converse is true of the spaces above and below the lower intercepted line.

Figure 10 will be recognized as a complex made up of 6 and 7. The line CD shows no illusion of the Poggendorff type. The lines AB and EF show the typical illusion. At O and N the intercepting parallels are somewhat extended, and it will be observed that the acute angles thus formed, instead of seeming

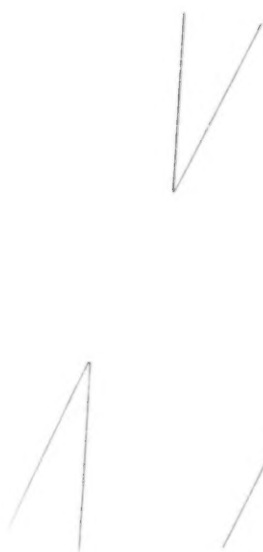


Fig. 6.

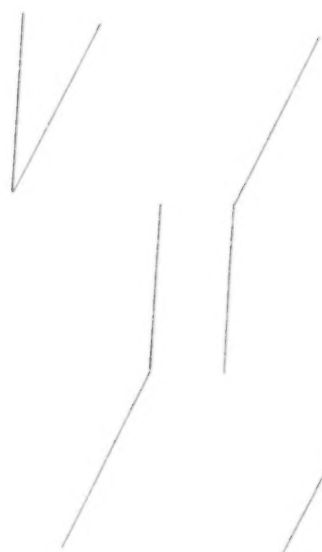


Fig. 7.

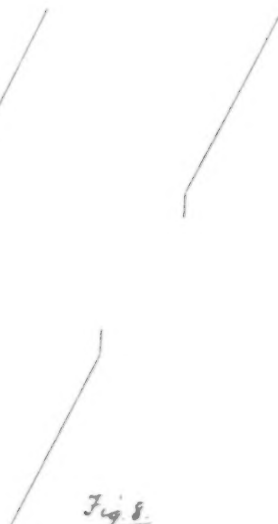


Fig. 8.

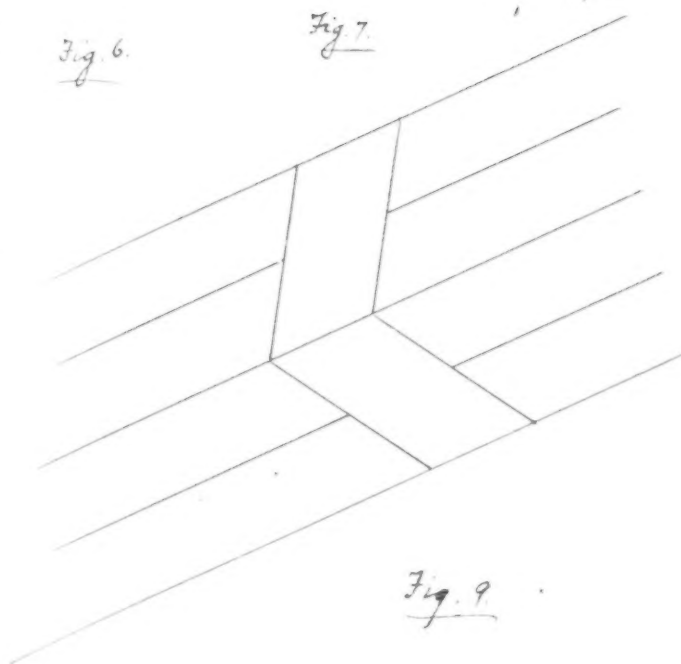


Fig. 9.

larger than the acute angles at 2 and 3, where no illusion appears, seem rather to be noticeably smaller.

In view of this negative evidence it seems clear that the false estimation of the angles cannot be regarded as the explanation of the Poggendorff illusion. The real causes of the illusion are to be looked for in some other direction. We turn for our investigation of the figure to one of the positions in which the illusion disappears. Such a position is that in which the intercepted line is horizontal. If the apparent length of the interval between the points of interception is compared with an equal interval marked off by intercepting parallels which are perpendicular to the intercepted line, it will be observed, as seen in Fig. 11, that the interval in the Poggendorff figure is underestimated. This underestimation was subjected to quantitative determination. The method employed in these investigations was the same as that used by Heymans.¹ Cards were arranged so as to present a pair of parallels perpendicular to the horizontal line which they intercepted. The distance between these parallels, or the standard distance, was 50 mm. At the right of the parallels just described was a second pair of parallels also intercepting the horizontal line, but sloping obliquely from the upper right to the lower left, forming an angle of 45° with the horizontal line. The extreme right part of the figure, including the right oblique line and the part of the horizontal lying to the right of it, was made movable, so that the subject could easily adjust the distance between the points at which the oblique lines intercepted the horizontal. The errors for three subjects are given in Table I.

TABLE I.

SUBJ.	NO. OF DETER.	AVG. ERROR.	M. V.
J.	10	6.7 mm.	1.1
E.	6	14.5	1.3
C.	3	6.3	1.1

The next step in the investigation was to break the figure up into its elements, with a view to discovering the importance of the different parts of the oblique parallels for the illusion.

¹ *Zeitsch. für Psych. und Phys.*, IX., 221.

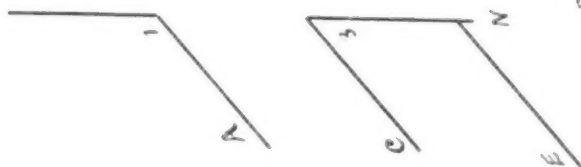
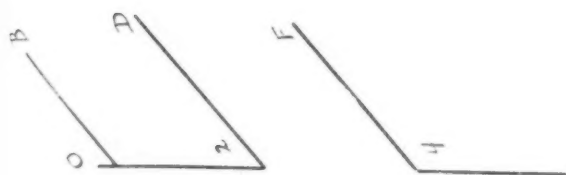


Fig. 10.

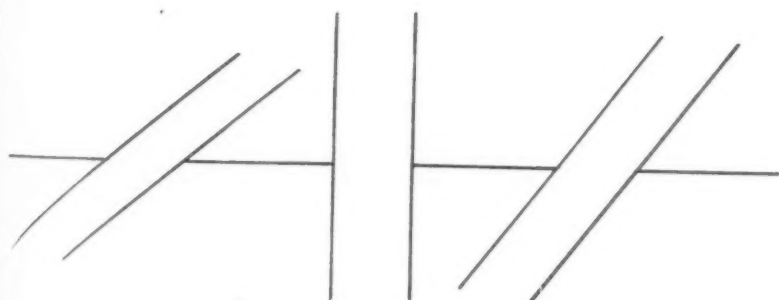


Fig. 11.

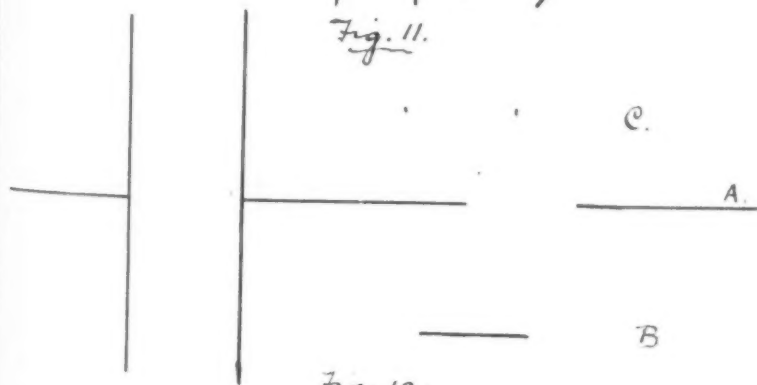


Fig. 12.

These oblique parallels were divided into four parts. The part of the left line above the horizontal was designated a; the part of the right oblique line above the horizontal was called b; in like manner the lower left line is c, the lower right line d. Cards were prepared which made possible all the different combinations of these lines, and measurements similar to those reported in Table I. were made with each combination. The standard distance was again 50 mm.; the angle of obliquity was as before 45° . The average errors are given in mm.

TABLE II.

NO. OF DETERM. IN EACH CASE.	SUB. J.		SUB. E.		SUB. C.		ORDER OF GREAT- EST ILLUSION.		
	10		6		3		SUB. J	SUB. E	SUB. C
	Avg.	M.V.	Avg.	M.V.	Avg.	M.V.			
LINES PRESENT.									
a	7.5	0.9	11.8	0.6	7.7	0.9	ac	ad	cd
ab	8.8	1.0	13.0	0.5	5.3	1.1	d	abd	abd
ac	9.9	0.9	11.8	1.2	5.3	3.1	ad	abcd	abc
ad	8.9	1.0	17.5	1.2	8.7	1.1	ab	acd	ad
abc	2.4	0.4	13.3	1.3	9.0	0.6	abd	abc	d
abd	8.2	1.0	16.1	0.9	11.6	1.1	a	ab	a
acd	7.2	0.7	13.3	1.0	7.6	0.5	acd	a	acd
abcd	6.7	1.1	14.5	1.3	6.3	1.1	abcd	ac	abcd
b	3.6	0.8	5.7	0.4	2.0	0.6	bd	bcd	bcd
bc	1.0	0.6	4.7	0.8	1.0	0.6	none	d	ab
bd	6.0	1.4	7.4	0.7	2.0	0.0	bcd	cd	ac
bcd	4.1	0.7	11.1	0.5	6.3	1.1	b	bd	b
c	2.9	0.7	5.3	0.7	1.0	0.6	abc	none	bd
cd	2.9	0.5	8.5	0.7	12.0	0.0	c	h	c
d	9.7	1.3	8.7	0.8	8.0	0.6	cd	c	bc
none	5.7	0.6	6.8	0.8	0.3	0.9	bc	bc	none

There are very noticeable personal differences in these results, but the general tendencies are common. The lines a and d are favorable to the illusion; the lines b and c are unfavorable. The figure resulting from the combination of the favorable lines is identical with the Müller-Lyer figure for underestimation; that resulting from the combination of the lines unfavorable to the illusion is identical with the Müller-Lyer figure for overestimation. Even under the last-named conditions, however, there is a slight underestimation as compared with the standard made use of in this case. This renders it necessary for us to examine at the outset of our discussion the

process by which the distance adopted as a standard in this case is estimated.

Fig. 12 makes it possible to compare the standard distance between the parallels with three other equal distances. The distance between the free ends of the interrupted horizontal (case A) is usually judged as shorter than the distance between the parallels. That this statement cannot be put in a universal form will appear when the results of the subject C, in Table II., are examined. The illusion in its general form has been observed by a large number of individuals who were not subjected to any quantitative tests. We are accordingly justified in regarding C as exceptional in this particular. This conclusion is confirmed by the fact that his judgment in case B is also exceptional. Most observers see a short unbroken horizontal line as equal to the standard distance. The subject C, on the other hand, sees the horizontal line as longer than the standard. Finally, when the interval between the parallels is compared to the interval between two dots (case C), the latter is usually seen as longer. The judgments in this case are, however, so irregular that quantitative determinations are of little value. Certain quantitative results which throw light on some of these cases are to be found in Tables III. and IV. In Table III. the results are given from comparison of the interval between the free ends of the interrupted horizontal and the interval between the parallels. The method is the same as before. The standard, that is, the distance between the parallels, was varied from 2.5 to 150 mm. Ten determinations were made in each case for J, five in each case for E.

TABLE III.

DISTANCE.		2.5	5.0	10.0	20.	50.	80.	100	150
Subj. J.	Avg.	1.4	1.9	3.3	3.8	5.7	8.2	3.0	-1.4
	M. V.	0.2	0.2	0.4	0.4	0.6	1.4	1.2	1.7
Subj. E.	Avg.	1.3	2.5	3.3	3.2	6.8	9.9	8.8	-0.4
	M. V.	0.2	0.0	0.2	0.6	0.8	1.5	2.5	1.8

Table IV. presents the results of a comparison by C of the distances marked off by a number of unbroken horizontal lines and the interval between the free ends of an interrupted hori-

zontal. The standard line in each case was the unbroken horizontal, the number of determinations five.

TABLE IV—SUBJECT C.

DISTANCE.	5.0	10.	25.	50	70	100
Avg.	2.0	5.8	6.0	10.2	11.2	14.0
M. V.	0.0	0.3	1.2	0.6	1.4	1.6

It will be noted that Tables III. and IV. are directly comparable. For J and E the interval between parallels is judged as equal to the horizontal line which is used as the standard in Table IV. The same general result is to be found in both tables. The illusion is greatest for short distances.

In connection with these facts attention is to be called to those cases in Table II. in which the free end of the horizontal is left after the withdrawal of both parts of the oblique. Take, for example, the case in which *d* alone is present. It will be seen that for J and C the illusion is about as strong as it is when *d* is combined with the most favorable line, namely, the line *a*. Again, in such cases as *a*, *ac*, and even *bd*, there are indications that the free end of the horizontal is favorable to the illusion.

If we attempt to find an explanation of the illusion which appears when the break in a horizontal line is compared with the interval between two parallels, or with the unbroken horizontal line, it is evident that we cannot appeal to the principle that filled space is overestimated when compared with empty space. The two intervals stand on a par with respect to their content. Then, again, when these intervals are compared with an interval bounded by dots, it will be seen that this last example of empty space is subject to overestimation rather than underestimation. The theory of Lipps, which attributes such illusions to the bounding activity of the various lines and points, on the general principle that the greater the bounding activity of the terminal lines or points the more the bounded interval is underestimated, does not seem adequate to explain these facts. Thus, it is difficult to see how the parallel lines can be regarded as less intense boundaries than the free ends of the interrupted horizontals. Again, in his explanation of the Müller-Lyer

figure. Lipps assumes that the oblique lines which slope outward have a bounding activity which is negative. This cannot hold when applied to case *bc* in Table II. To be sure, some of these difficulties could be avoided if the bounding activities were regarded as applying to the intercepted lines rather than to the intervals. But this brings us back to the position taken early in this paper, namely, the position that the points at the end of a line have spatial relations in all directions, and any modification of the relations in one direction involved at the same time the opposite modification in the opposite direction. Such shifting of points cannot be explained by the bounding activity of lines or points, for it is the boundaries themselves that are shifted in their spatial relations.

The movement hypothesis, on the other hand, seems to meet the demands of all the different cases. The more intense the sensation of movement, the greater will be the estimation of the distance; conversely, the less the intensity of the sensations of movement, the shorter the estimated distance. If from a given point the tendencies of movement are outward, then the movement outward will be, if it is executed, somewhat easier. In any case the tendency will result in an active tension of the muscles which favor outward movement of the eyes. The space lying in that direction will be underestimated, while space in the opposite direction will be overestimated. The point from which these tendencies emanate will, accordingly, be shifted outward. If, on the other hand, the tendencies from a given point are inward, the point will be shifted inward. Influences of one kind and another may be combined in great variety of quality and intensity. The final apparent position of a point will be determined by all of these influences operating together. The question now arises: What are some of the influences which give rise to tendencies of movement? The most important fact in this connection is that the eye tends to follow lines rather than to direct its own course through space. When lines are present in the field of vision they tend to attract and direct the eye in its movements or in its tendencies of movement. But every line has two directions, and, therefore, it cannot in itself determine the particular direction in which the eye is to move or

to tend to move in any particular case. There enters at this point of our discussion a very important and very ambiguous factor. We may designate it by the convenient term, the direction of attention. By this we mean to indicate that the particular direction of movement in any given case depends on the relation of that part of the field of space which is subjectively the most important to all other parts. Thus, in the cases reported in Table II. the important distances were the breaks in the horizontal lines. The tendencies of movement originating in all of the lines will have their direction determined by their relations to these two intervals. If we attempt now to discover which lines are favorable to movement across these intervals and which are unfavorable we shall find full agreement between our results as recorded in the tables and principles just developed.

In Table II. it will be seen that all those combinations of lines which render movement across the open interval easy, as, for example, a, ad, acd, dcb, and others, are favorable to the illusion. The fact that cb is not a case of overestimation rather than underestimation can be explained by a consideration of all the facts reported in Tables III. and IV., together with some of the subjective observations made during those tests. A characteristic observation was made by C while comparing the horizontal lines with the interval between the free ends of an interrupted horizontal (Table IV.). The ends of the lines and of the interval seemed more or less uncertain. The ends of the line seemed to run out into the surrounding space, thus making the line seem longer; the ends of the lines bounding the interval, on the other hand, seemed to close in on the interval, making it seem smaller. In both these cases there is evidently a strong tendency for the movement which has originated in the line to extend itself beyond the end of the line. This holds for those cases reported in Table II., in which the free end of the horizontal is favorable to the illusion. To return now to the case bc. While these lines alone would result in overestimation of the interval as in the normal Müller-Lyer figure, their influence is counteracted by the tendencies produced in the horizontal lines.

The principles may also be applied to the estimation of the interval between the dots as compared with the interval between

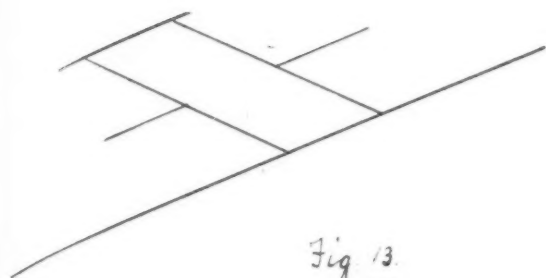


Fig. 13.

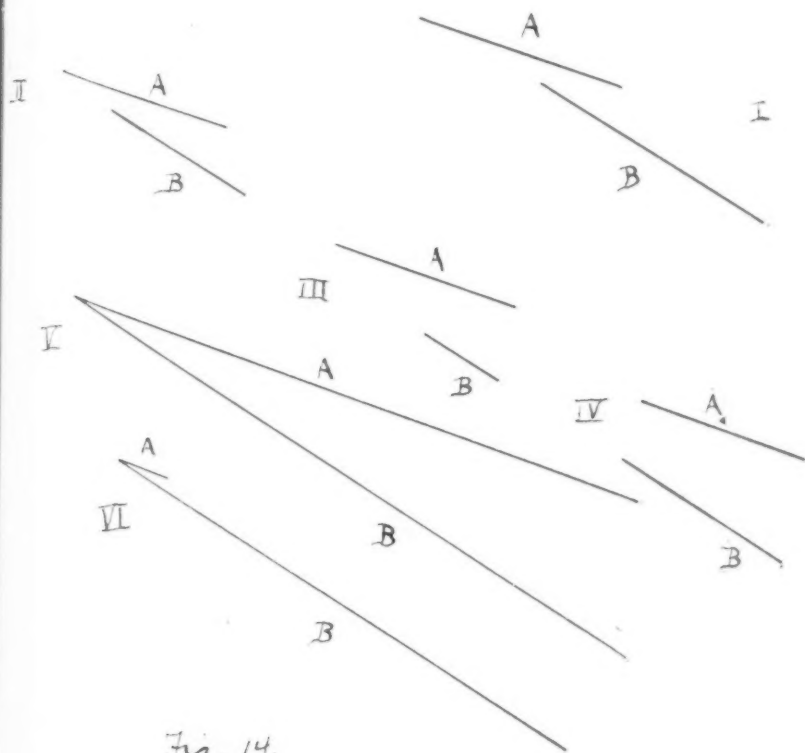


Fig. 14.

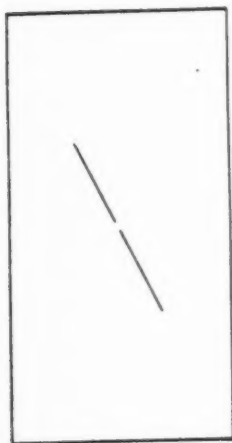
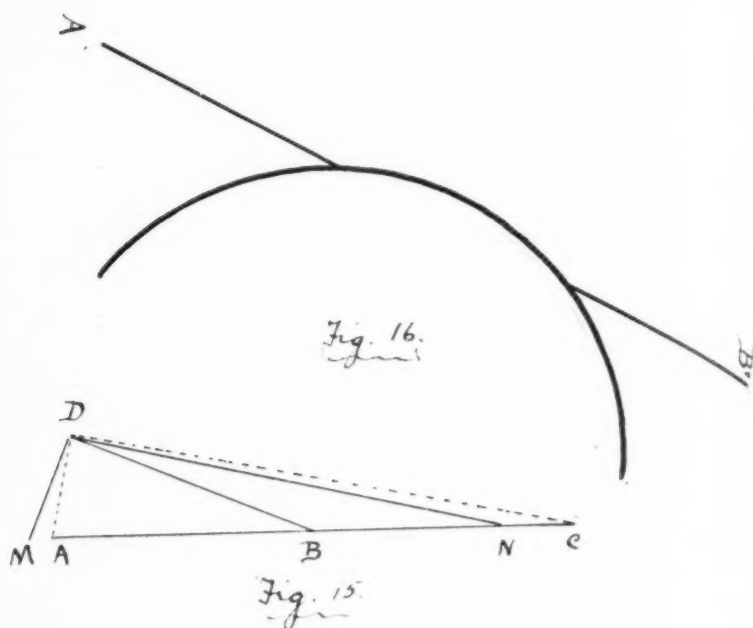


Fig. 17.

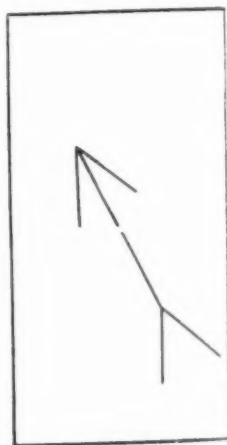


Fig. 18.

the parallels. The dots give rise to no tendencies of movement across the interval. For most observers the perpendiculars counteract the tendencies toward underestimation which originate in the horizontal lines. The influence of these horizontals is not entirely overcome. In the case of the subject C it seems to persist in its full intensity. In fact, his description of his method of estimation would seem to justify the special explanation that he neglects the parallels almost entirely.

The measurements and explanations thus far presented apply only to the exceptional positions of the Poggendorff figure; to those, namely, in which the typical illusion disappears. That the underestimation of the interval between the points of interception is present, though in a smaller degree, in other positions of the figure, can be seen by comparing those cases in which the illusion appears with those in which it does not appear, as given in Figs. 6, 7, 8 and 10. But this fact does not suffice to explain the appearance of the illusion in certain positions and its disappearance in others. There is another fact of false judgment to which attention was called in the discussion of Fig. 9. The distances along the parallels (in the case of Fig. 9 the vertical distances) are also misjudged. For the explanation of this misjudgment we have only to refer back to our earlier discussions. When horizontal or vertical distances along the parallel lines are the subjects of attention, as they are in the usual positions of the Poggendorff figure, those portions of the parallels lying on the obtuse-angle side of the intercepted line will be overestimated. The overestimation of this distance along the parallels with the underestimation of the oblique distance across the interval gives us a full explanation of the illusion. At the same time we have in the appearance and disappearance of the typical Poggendorff illusion in different positions of the figure an illustration of the fact that lines produce illusions only when they have some direct influence on the particular direction to which the attention is turned. When the Poggendorff figure is in such a position that the intercepted line is horizontal, the false estimation of distances along the parallels has no direct bearing on the distance to which the attention is directed. The whole influence of the parallels is there absorbed in aiding the

intercepted horizontal line in carrying the eye across the interval. When, on the other hand, the length of the parallel becomes itself a matter of judgment, as it does when the parallels, instead of the interrupted line, are vertical or horizontal, then the whole combination of conditions changes. The influence of the intercepted line is such that distances along the parallels are overestimated on the obtuse-angle side of the point of interception. The parallels no longer bridge over the interval so fully, and the underestimation is, therefore, much less marked.

In view of all these considerations, both negative and positive, we conclude that the Poggendorff illusion is not due to the false estimation of angles. The question now presents itself: Can the Poggendorff illusion under any conditions give rise to a false estimation of angles? Fig. 13 furnishes empirical evidence which goes to show that it can. The intercepted line in this figure does not seem parallel to the other oblique lines, but slopes in such a way that it seems to meet the lower line at the left and the upper on the right. This leads to a discussion of the general question: How do we in any case judge the size of angles?

The discussions of the estimations of angles have always confined their attention to the simplest case of such judgments, namely, those in which the vertex is expressed in the figure. Such a limitation of the discussion is obviously unwarranted and has led to theories of angles that are correspondingly shortsighted. Angles may appear, and often do appear, in concrete experiences between lines and surfaces not in direct contact. How is it possible for us to estimate these angles? Fig. 14 presents a number of cases in which lines were so drawn that each pair subtends exactly the same angle. All the lines marked A are parallel and all the lines marked B are parallel. The difference in apparent size of the angles is noticeable at once.

By means of these figures let us test Wundt's hypothesis, which is that small angles require relatively greater energy for the eye to move through them on the general principle of physical and physiological inertia. This greater energy of movement is interpreted as due to greater surface between the

sides. Compare now the two Angles IV. and VI. at the bottom of the figure. The left-hand figure shows an angle to which Wundt's theory should apply most admirably and we should have overestimation. On the other hand, the lower right figure should be beyond the range of application of Wundt's theory and we should have underestimation. The appearance is, of course, directly opposed to the theory in both cases.

Or take the Helmholtz theory of contrast in direction of movement, it is difficult to see how the greater and weaker effects of contrast could explain the differences in the estimations of the angles in Fig. 14 for which the vertices are not expressed. Yet there is a very notable difference in the judged sizes of these angles. The general criticism of perspective theories seems to the present writer to have been so fully carried out by Wundt that there is no call at this time for a repetition. The angles in Fig. 14, it may be noted, are so drawn that perspective influences play little, if any, part. All the lines are in the same direction and the lines are drawn from the vertices obliquely towards the observer.

The most obvious induction from Fig. 14 is that the judgment of the length of the sides of an angle is a very important factor in the judgment of the size of the angle. The *a priori* probability which attaches to this statement is so great that it seems hardly possible that it should have been persistently overlooked. In addition to this judgment of the length of the sides, there must also be the judgment of the distance between the sides at a certain distance from the vertex. In short, the whole process is a kind of triangulation in which the mind takes into account three factors, namely, the distances from the vertex at which the measuring arc is to be drawn, and the length of that arc with respect to the whole circumference of the circle. Thus it is possible for us to recognize an angle whether we measure the arc near its vertex or at some distance from the vertex. Such complex judgment is, however, subject to many influences that produce illusion. If, for any reason, the arc is judged too long, the angle is overestimated, or, *vice versa*, a judgment which makes the arc too short results in underestimation of the angle.

Misjudgment of the length of the sides may lead indirectly to such false estimation of angles. Thus, if the subject is asked to point out the vertex for Angle IV. in Fig. 14, he will usually place it too far from the lines, that is, he will underestimate the length of the sides. Conversely, in Angle I., he will place the vertex on A too near the left-hand extremity of B.

These facts apply with less clearness to Angles V. and VI. VI. is underestimated, but the reason in this case may be that the arc is underestimated rather than the length of the sides misjudged. Explanation of Angle VI. is difficult. The principle discovered in the other cases, however, is of very general importance. It can be brought into direct relation with the fact that all acute angles are overestimated and obtuse angles underestimated. This will appear from Fig. 15. AC is a line at the middle point of which (B), an oblique line equal in length to AB is drawn. Since movement from B to A is favored by BD , it follows, on the principle developed, that the point B will be shifted towards A . As compared with BD the line BA will be underestimated, and the line BC will be overestimated. In comparing the two angles DBA and DBC the arcs will, therefore, be estimated as cutting AC , not at A and C , but at some points as M and N . The true arcs (or chords) of measurement are DA and DC , but the chords used are DM and DN . And since DM is longer than DA , while DN is shorter than DC , the angle DBA will be overestimated, while the angle DBC is underestimated.

In this way it is possible to explain all the angle illusions. The movements upward and downward of the lines in the Zöllner pattern, as observed and described by Helmholtz, are direct confirmations of this position. The successive fixation of certain points in the figure brings out the illusion of length, first on the side of an acute angle, then on that of an obtuse angle. The angles do not suffer any further change by such successive fixation, but the presence of a neutral point of reference gives clearness to the illusions of length, that is, the original source of the angle illusion appears in its simplest form. Furthermore, it will be found that if Wundt's and Hering's modifications of the Zöllner figure are so drawn that the extremities of the

different oblique lines are not distinctly marked (particularly the point to which all converge), the illusion of bending in the horizontals will be very much reduced, if not entirely lost.

Finally, Figs. 16, 17 and 18 present angle and distance illusions in such relation that the principles discussed may be directly applied and at the same time confirmed. In Fig. 16 the line *AB* seems to be bent inward so that its two parts if continued would form a very obtuse angle within the circle. This is due in part to the underestimation of the sides of the acute angle and the overestimation of the arc between the points of interruption. Furthermore, the rapid increase in the distance between the line and the circle beyond the points of contact may result in overestimation of the arc by which the angle would be estimated. In Figs. 17 and 18 a part of the diagonal is drawn in such a way that it extends for equal distances on each side of the middle of the rectangle. In Fig. 17 it will be noted that the part of the diagonal seems to meet the left side of the rectangle below the corner, and the right side above the corner. The illusion will be clearest if the ends of the line are fixated. It will also be observed that there is a marked tendency for the eye to pass in its movement, not through the longer distance from the ends of the line to the corners, but from the ends of the line to the nearer right and left sides of the rectangle. This tendency is a full explanation. It leads to the angle illusion through the underestimation of the side. Fig. 18 combines a number of the facts already pointed out. The detailed analysis may be left to the reader.

THE NATURE OF ANIMAL INTELLIGENCE AND THE METHODS OF INVESTIGATING IT.

BY PROFESSOR WESLEY MILLS.

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Those interested in this subject may be classified in the main somewhat as follows perhaps :

1. Those who see in the animal mind only a sort of weaker human intellect ; who look chiefly for evidences of intelligence and take no account of the failures and stupidity of animals.

2. Those who recognize that the animal mind is not the equivalent of the human mind in all its qualities as it exists in men of superior development in the highest civilization, but who nevertheless recognize the resemblance up to a certain point to man.

3. Those who approach more or less closely to the view that animals are automata, or at all events consider animal consciousness as utterly different from human consciousness, except in a few of its lowest states. With regard to investigation or material of knowledge we recognize a class who, while suspicious with reference to the conclusions of the anecdotal school, do not consider anecdotes worthless, much less meriting the supreme contempt some writers manifest for such evidence. They believe that there is no more reason to set aside reliable anecdotes of animals than of men. Anecdotes may illustrate a normal, sub-normal or super-normal mental condition or development ; but if they set forth facts it is for the psychologist to explain, not to ignore them. Another class of investigators see little or no good in anything in comparative psychology or psychology in general, except experiment, which is for them the sole key to a reliable knowledge of the mind.

Among psychologists as among biologists there are those who are willing to shut themselves up in the narrow lane of experi-

ment—a lane with high walls on each side cutting off all view of the surrounding domain open to general observation and experience. As these people see so little themselves yet ever behold that little before them, they come to interpret everything in the light of their own limited observations. They insist on others believing as they do; they would have others wear the fetters they have put on themselves; all thinking must conform to the rigid conditions in which they are content to live and move and have their intellectual being.

The only hope of safety for the man who engages in experiment is ever to check his observations, and, above all, his conclusions, by other wider observations and those broad general principles which are like the points of the compass to the mariner; and I venture to suggest that it is the failure to do this which accounts for the greater part of the wrecks scattered along the shores and over the bottom of those seas traversed by the experimenter in biology and psychology.

As we have had what I cannot but think a recent conspicuous example of the sort of neglect referred to, I propose to criticise the methods pursued and the conclusions drawn, the more especially as this investigator claims to have swept away, at one fell swoop, almost the entire fabric of comparative psychology.¹ He appears to believe that he has razed the old structure to its very foundations and settled once and forever the weightiest problems with which others have been long struggling in vain.

Dr. Thorndike has not been hampered in his researches by any of that respect for workers of the past of any complexion which usually causes men to pause before differing radically from them, not to say gleefully consigning them to the psychological flames. For Dr. Thorndike the comparative psychologists are readily and simply classified—they are all insane—the only difference being the degree, for he speaks of one of them as being ‘the sanest’ of the lot.

Having thus cleared the way, this investigator proceeds to set forth, in no uncertain terms, what we should believe, and his creed is very brief and easily remembered. Animals neither

¹ ANIMAL INTELLIGENCE, by Ed. L. Thorndike. (Monograph Supplement to the PSYCHOLOGICAL REVIEW, Vol. II., No. 4, whole No. 8.)

imitate, feel sympathetically, reason nor remember, though about the latter point he is not quite so dogmatic.¹ He comes very near to the belief that they are automata pure and simple, though this he does not assert in so many words. The above mentioned views he thinks he has deduced from experiments. If so, the present writer thinks so much the worse for the experiments. At all events, with the exception of reasoning about which I wish to reserve judgment, I have come to widely different conclusions and from experiments also as well as from other sources of information.

Dr. Thorndike in criticising my book² has given the impression that I have not made experiments, or 'crucial experiments.' Now, I think it can be shown from my publications that I have recorded more experiments (not to mention scores which have not been described) than all other investigators together, if we except those working on insects. Moreover, these experiments have been invariably conducted under natural conditions, the absence of which seems to be almost a recommendation with some, but which I consider a fatal objection to Dr. Thorndike's work. Incidentally, I may remark that a laboratory as ordinarily understood is not well suited for making psychological experiments on animals.

When Dr. Thorndike charges that most of the books do not give us a psychology, but rather a eulogy of animals; that they have all been about animal intelligence, never about animal stupidity, I recognize a certain amount of truth in the imputation. But I beg to suggest that to a certain extent the same applies to works on human psychology. To what extent has the mind of the savage or semi-barbarous man been investigated? Yet to make comparisons between man and the lower animals parallel such a study is essential. I do not find Dr. Thorndike's publication any freer than others from the fallacies aris-

¹ In an account of his own work given by Dr. Thorndike in *Science* (Vol. VII., p. 823) he goes still further in his negations. "Conception, inference, judgment, memory, self-consciousness, social consciousness, imagination, association and perception, in the common acceptation of the terms, are all absent from the animal mind."

² *The Nature and Development of Animal Intelligence*. London, T. Fisher Unwin; New York, The Macmillan Company. 1898.

ing from considering the superior class of human minds or the civilized and educated man, and comparing him with the lower animals. Dr. Thorndike considers his experiments crucial; that individual peculiarities have been eliminated; that hunger is an adequate stimulus or condition; that no personal factor need be considered; that "the question of whether an animal does or does not form a certain association requires for an answer no higher qualification than a pair of eyes"—all of which I consider fallacious and to a large degree explanatory of the misleading psychology which he has constructed. With dogs I found several stimuli stronger than hunger, as any one really acquainted with the nature of animals must know, and such stimuli may, and frequently do, lead animals so to deport themselves that they become a perfect revelation to those who have long been associated with them.

I had that well illustrated in the case of a tame fox (vixen) that I reared. When a certain critical period (*œstrum*) was reached her whole nature took on a new character, and it became practically impossible to control her as formerly; and, unless I had ocular demonstration of the facts, I would not have believed it possible for any animal to have accomplished what this fox did. Nevertheless, in order to learn her methods of procedure it was necessary to observe unawares to her, and that I may say applies to very many studies of animals. That a pair of eyes is not all that is requisite for a complete outfit as an observer, Dr. Thorndike's work but too pointedly exemplifies. I venture to think that in all cases it is a question of whose eyes, or, in other words, the training those eyes have had, and still more of the intellect that passes judgment on what is seen.

I have all along endeavored to emphasize the importance of individual differences. They do somewhat disturb statistics, and they rather spoil curves, it is true, and experimenters have always been prone to ignore them; but they exist in nature, and when adequately recognized our explanations for many things will be found altogether too simple, and, therefore, delusive, rather than real and adequate.

Dr. Thorndike admits that 'an act of the sort likely to be attended to will be learned more quickly.' Undoubtedly, yet

this investigator has practically ignored this in his tests, for he placed cats in boxes only $20 \times 15 \times 12$ inches, and then expected them to act naturally. As well enclose a living man in a coffin, lower him, against his will, into the earth, and attempt to deduce normal psychology from his conduct.

The present writer has pointed out distinctly that when animals are removed from even their usual, not to say natural, surroundings they may be so confused or otherwise diordered that they fail to act normally, and this I have illustrated by experiments. Dr. Thorndike found that dogs when placed under similarly improper and disturbing conditions, as I deem them, behaved in a like panicky way, except that they gave up sooner, which he attributes in part to their being insufficiently hungry. But dogs have not as much perseverance as cats, as my experiments abundantly prove. However, had Dr. Thorndike witnessed the resources of my dogs when let loose in the yard after some of their companions, which had already been set free in the adjoining fields and woods, I can believe that even one so fast bound in the grip of his experiments as he would have altered his opinions on this and many other subjects. In dogs under such circumstances we have illustrated not alone an adequate motive or stimulus, but it is shown that they have memory—can conjure up exciting pictures of the pleasure-giving scenes of the past, re-experience in some fashion the delights associated with that past, make a sort of generalized abstract of the whole—in a word, have very much the same experiences as the human being who accompanies them and delights in such things.

When the contrary is proved by adequate observations or experiments, I am ready to alter my opinions, but not on such evidence as seems to go directly counter to all that one has borne in upon him by daily observation. To do otherwise is, indeed, to bid adieu to common sense as well as to science, and to accept as proof what seems to me of no more value than counterfeit coins, but which, nevertheless, like bogus money, deceives the unwary, even among psychologists.

The experiments on chicks I consider the least misleading and most valuable part of Dr. Thorndike's work. Not only are birds much lower in the psychological scale; not only does free

association explain more in their case, but the conditions of the experiments were rather more natural. A pen $16 \times 14 \times 10$ makes for a chick a very different thing from one $20 \times 15 \times 12$ for a cat. Even those curves which in the case of the cats and dogs only serve to stereotype error are possibly of some value when applied to the chicks. Says Dr. Thorndike: "I hate to burden the reader with the disgusting rhetoric which would result if I had to insist on particularizations and reservations at every step." If anything, just such particulars might have somewhat redeemed these experiments. They might at least have proved helpful in some way. At the present stage of comparative psychology we are in need of observations down to the minutest details. We can better spare the rhetoric.

When we consider how widespread—indeed, almost universal—is imitation among animals of the middle and higher grades, that it is difficult so to separate it from the general psychic life of the animal as to be able fairly to analyze their mental processes and determine how much is due to independent development *per se* and how much to imitation, one cannot but marvel at the degree to which that magic word of modern science 'experiment' can blind the mind to facts thick as the leaves of the forest, and all pointing to the importance of imitation in animal life. So obvious an example of imitation as the talking of parrots is set aside or twisted out of all recognition. It is, moreover, a case of heads I win, tails you lose. Much that Dr. Thorndike has said when discussing this subject is valuable as suggesting a basis for observation and on the genesis of imitation, though this applies also to human psychology. There is one fallacy that underlies the whole of Dr. Thorndike's experimenting and vitiates his conclusions, namely, this: that he overlooks the many possible and actual inhibitions to response to a stimulus. One would have thought that the case of the cat mentioned by him (p. 59) would have given him pause. The conduct of that cat, like all the rest, only proves to him that animals do not imitate.

I find myself ever disposed to imitate in certain cases, yet do not. To illustrate—when I read a chapter on psychology written in the fascinating style of James, one exemplifying the profundity of a Ladd or a Hall, the bold constructive character of a

Baldwin, or a vigorous plea on behalf of modern psychology by Cattell—the list might be much enlarged—I am filled with admiration, and there is an impulse to imitate, but I have not as yet taken the first step. Having thus been the subject of experiment in this way over and over again, I should, according to the logic of Dr. Thorndike, be characterized as a non-imitating creature—not only as regards the subject in question, but generally. The truth is far from this. There is a strong tendency on my part to imitate, but there are stronger forces acting to inhibit the process, and, moreover, these forces are not always the same nor is each always equally potent. In truth, the whole matter is very complex even in animals. I find no difficulty whatever in explaining why the animals did not respond to the stimuli Dr. Thorndike used.

When one meets the *questionnaires* he seems at last to strike the rock bottom of common sense. The author of the experiments referred to has no high opinion of the trainers. "I would first adjust all things in connection with the surroundings of the cat so that they would be applicable to the laws of nature, and then proceed to teach the trick." I see much saving sense in this remark, and believe that had Dr. Thorndike grasped its significance he would have given us a very different psychology. The writer seems to have totally neglected the methods and experience of the trainers of dogs for field work, and has also I believe failed to make use of the lessons the trainers of trick animals can teach us. Even to witness a performance of trick animals is enough to enable one to see how at one time the tendency to imitate assists and at another mars the performance. To be sure, there is a sort of deliberate, studied, high-class imitation possible to man, but beyond the reach of animals, but this is, after all, comparatively rarely employed in the lives of the great mass of men.

A student of McGill University has communicated to me the fact that a kitten which could not be induced to jump over an object placed before it did so only after seeing the mother do it, and after that there was no more trouble in getting it to perform the trick. The young hounds of the Montreal Hunt Club are taught by being actually put through the performance, *i. e.*,

they are attached to an old and strong dog while hunting, so that Dr. Thorndike's contention as to the uselessness of an animal's being put through a performance breaks down. Indeed, that was to be expected even from his own teaching as to the genesis of associations, to go no further. As to the inability of animals to have memory images for which Dr. Thorndike contends I find myself, in the light of my experience with animals, quite unable to agree. I believe that their memory is like our memory of the same things so far as image, etc., are concerned, but that there may be with man, owing to the complexity of his mental condition, a more varied fringe around that memory core which latter will be much alike in both the man and the animal.

To refer to but a single experiment to illustrate this: I had a greyhound that was very prone to chase cats, a habit which became with him more and more pronounced, I presume, from his success in consequence of his speed. On the occasion I wish to emphasize I had taken the dog in a certain direction, and, as a result, a cat crossing the street was so hotly pursued by him that she took to a tree. Many months after I brought the dog along this same way, but approached the scene of the exciting chase from the opposite direction. Long before the exact spot was reached the dog was all attention. It was perfectly plain that he remembered the long-past incident, and that certain feelings (which accompanying feelings Dr. Thorndike denies to animals) were also aroused; but great was my astonishment when the dog stopped at a certain tree, looked up and behaved otherwise in such a manner as left no doubt in my mind that he remembered the identical tree and every detail of the whole incident. This cannot be explained by the sort of consecutive association that Dr. Thorndike would substitute for 'memory' as ordinarily understood, for the locality was approached from the opposite direction.

The central phenomena of memory were in this case the same with the dog and his master, but the feelings and the mental fringe or associated ideas were not identical. In the one case they were appropriate to the dog, in the other to the man, his master, who was in this instance trying to draw some psychological conclusions, so the difference was considerable; but had

it been a hunting expedition in which both dog and man took an active part, the resemblance even in revival would have been altogether greater.

One finds in the end, however, that Dr. Thorndike does allow representation to animals within very narrow limits. Along with this writer's "I never succeeded in getting the animal to change its way for mine," a quotation from a recent interesting and instructive publication seems timely: "One must be familiar with the normal conditions of the insects in question before he is able to note those slight changes in the environment that offer some opportunity for an adaptation of means to ends, or before he is competent to devise experiments which test their powers in this direction."¹ The above seems to the present writer to be applicable in the widest sense to investigations in comparative psychology.

The experiments to which Dr. Thorndike refers under the heading 'Association by Similarity and the Formation of Concepts' only really show that animals may react to a vague stimulus, and this is quite sufficient to meet the ends of their existence in many cases; but neither these experiments nor any others show conclusively that this alone is the best of which animals are capable. The comparison of animal consciousness to human consciousness during swimming is open to the same objection. Such a mental state is possible to both man and animals, but neither is confined within such narrow limits of almost pure sensation.

I must object to Dr. Thorndike's analysis of human consciousness in playing open-air games as being inadequate. It does not correspond with my own experience nor with the accounts I have heard persons of different degrees of skill give as to what was going on in their minds during the playing of games. No doubt Dr. Thorndike's account does fit a certain portion of the mental phenomena, but the whole matter is much more complex than he seems to think, and is worthy of an analysis more accurate and comprehensive than has ever been given to it. Such views of animal consciousness as Dr. Thorndike presents seem

¹ *Instincts and Habits of the Solitary Wasps*, by Geo. W. Peckham and Elizabeth G. Peckham, p. 234.

to me altogether too narrow to meet the actual mental condition of, say, a dog when engaged in a fowling expedition.

From certain experiments which I made with my dogs in play, taken along with scores of others, I find myself utterly unable to agree with many of the views of the destructive or narrowly restrictive school of comparative psychologists. We should surely be very cautious in denying wholly to animals what Dr. Thorndike terms 'free floating ideas.' The believer in evolution will demand that, in this and other cases in which qualities man possesses are denied to animals, there be the clearest proofs given. The burden of proof lies with those who deny them, and this remark applies to feelings as well as intellectual processes, though to a less degree. Nor can I agree with those who maintain that we must always adopt the *simplest* explanation of an animal's action. Such does not apply to man, and why should it meet every case among animals? Though in this regard Professor C. Ll. Morgan with others seems to me to be in error, I fully agree with the views of this writer as quoted in the publication under consideration (p. 86): "Lastly, before taking leave of the subject of the chapter, I am most anxious that it should not be thought that in contending that intelligence is not reason I wish in any way to disparage intelligence," etc. But Professor Morgan is more and more in sympathy with the destructive school, so that he now seems willing to surrender anything to all and sundry who may ask him to stand and deliver. I have been myself classed by one of my reviewers¹ with Romanes. While I agree with much in Romanes' attitude in regard to animal intelligence, nevertheless, since this writer preferred to work upon second-hand material rather than make observations and experiments for himself, and had, moreover, a tendency to speculation rather than the accumulation and weighing of facts, I prefer to be myself considered an humble follower of Darwin, who, so far as he went in animal psychology, best illustrates the method and especially the spirit that will, I think, prove most fruitful.

The one point about which I feel like withholding an opinion till many more observations have been made is that of reasoning.

¹ *Science*, Vol. VIII., p. 520.

That animals can reach C by some mental process when A and B are given, and that this is to be explained either by some process of inference or by one as yet unexplained, I have little doubt. Unquestionably, association explains much in the mental structure of man and still more in animals, but that this is the whole story when we get beyond elementary chapters in instinct I cannot for a moment believe, unless the meaning of the word is greatly and unwisely extended. The subconscious must enter largely into the psychic life of animals, as of men, and one who observes animals long and closely must believe that no such naked skeleton as Dr. Thorndike presents to us can represent the animal mind.

The mental processes of an animal are generally not comparable to pure tones, but rather like those tones that abound in overtones, though this applies still more to man. Our age will probably be looked back upon as one characterized intellectually by great destructive and constructive activity, but also as one readily satisfied with unduly simple explanations put forward with a confidence and rashness that will be astounding to a later age. As showing, however, a different spirit and tendency I quote the following¹ with much gratification, coming, as it does, from two most patient, sympathetic and successful observers: "Our study of the activities of wasps has satisfied us that it is impossible to classify them in any simple way. The old notion that the acts of bees, wasps and ants were all varying forms of instinct is no longer tenable and must give way to a more philosophical view. It would appear to be quite certain that these are not only instinctive acts, but acts of intelligence as well, and a third variety also—acts that are probably due to imitation, although whether much or little intelligence accompanies this imitation is admittedly difficult to determine. Again, acts that are instinctive in one species may be intelligent in another, and we may even assert that there is considerable variation in the amount of intelligence displayed by different individuals of the same species."

The same may, I believe, be affirmed for animals generally; and it is work of the character described in the monograph

¹ Op. cit., p. 228.

from which I quote which really advances comparative psychology.

Were it possible to observe an animal, say a dog, from the moment of its birth onward continuously for one year, noting the precise conditions and all that happens under these conditions, the observer being unnoticed by the creature studied, we should, I believe, be in possession of one of the most valuable contributions it is possible to make to comparative psychology. This would imply not one, but several persons giving up their whole time, day and night, by turns, to such a task. As yet, but very imperfect approaches have been made to anything of the kind; nevertheless, such as they have been, they are the most valuable contributions thus made, in the opinion of the present writer, and the more of such we have the better.

If to such a study another were added, in which the effect of altering conditions from time to time with the special object of testing the results on an animal or animals similarly closely observed from birth onward, we should have another most valuable contribution to comparative psychology; but experiment on animals whose history is unknown must, in the nature of the case, be very much less valuable than in such an instance as that just supposed.

As Professor Groos has suggested in a private communication to me, it is important to make observations on wild animals, and there seems to be room for the worker in comparative psychology in zoological gardens as well as in the field or forest. But I must again maintain that it is fact rather than theory—observation, as ordinarily understood, and experiment—that are more needed than anything else as yet.

RÉSUMÉ.

Comparative psychology is advanced rather by systematic observations and experiments than by anecdotes; nevertheless, the latter, when strictly true, are not valueless.

The study of the development of the animal mind (genetic psychology) is of the highest importance.

Insufficient attention has been paid to distinguishing between normal, subnormal and super-normal comparative psychology;

an objection, however, which applies with a certain degree of force to human psychology.

In making experiments on animals it is especially important that they should be placed under conditions as natural as possible. The neglect of this is a fatal objection to the work of the author of 'Animal Intelligence,' published as a monograph supplement to the *PSYCHOLOGICAL REVIEW*, Vol II., No. 8, 1898.

The portion of this research referring to chicks is the most reliable, and the suggestions as to pedagogics, etc., valuable.

This investigator's experiments show that certain associations may be formed under conditions highly unnatural, which associations bear about the same relation to the normal psychic evolution of animals as the behavior of more or less panic-stricken or otherwise abnormal human beings does to their natural conduct.

It is not proved, as asserted in the publication referred to, that animals do not imitate, remember, have social consciousness, imagination, association, and perception; nor that their consciousness is only comparable to that of a human being during swimming or when playing out-door games, as understood by this writer.

It is highly probable that animals, even the highest below man, have only rarely and at the best but a feeble self-consciousness, if it exist at all.

But on this point and on the question of inference, reasoning, etc., the time is not yet ripe for positive assertions.

It seems more than probable that the mental processes of the highest animals are not radically different from those of men so far as they go, but that the human mind has capacities in the realms both of feeling and intellection to which animals cannot attain. While it is desirable to push analysis as far as possible it is safer to remain in the region of the indefinite, to refrain from making very precise and positive statements as to whether the animal mind does or does not possess certain powers, till we are in possession of a larger storehouse of facts, especially of the nature of exact and systematic observations (or experiments). *Festinate lente* is a good rule to observe in regard to conclusions in comparative psychology.

THE DEVELOPMENT OF VOLUNTARY MOVEMENT.

BY PROFESSOR E. A. KIRKPATRICK.

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The work of a train dispatcher who has to direct the movements and stoppages of a few score of trains so that there will be no delays or collisions is justly regarded as very difficult, and the brightest minds must go through years of training before they are equal to the task. The child, however, who gets up from his play and brings us a book, and then resumes his seat, performs an act of much greater complexity and nicety of adjustment, for as large a number of muscles as trains are moved, and an impulse passes to and from each muscle; all these movements and adjustments take place in a few seconds, and a variation of a fraction of a second in the order of contraction interferes with the grace and accuracy of the movements as much as the variation of a fraction of an hour in the time of trains interferes with their successful movement.

Without previous practice, pigs, chickens and many other animals can coördinate visual sensations and movements so as to walk or run, avoiding obstacles and adapting themselves to the nature of the ground. Young chickens can move towards and pick up food with only a little less accuracy than adult chickens. The human infant has not such power of motor control at birth, and our problem is to determine how he comes into possession of it within a year or two. *First*, it is popularly thought that he learns how to make the movements; *second*, it may be claimed that the power to make such movements is inherited, just as it is in the case of the chicken, except that the mechanism is not complete for some time after birth, as is known to be the case with birds as regards flying; *third*, it may be claimed that the movements are partially provided for by the inherited mechanism and partly acquired and learned.

As to the first supposition, the evidence is overwhelmingly against the possibility of such a stupendous task being performed by a child in the short space of a few years. He has over four hundred muscles, and these may be combined in practically an infinite number of different ways. If it depended entirely upon chance or the child's ingenuity whether he should find the right combination for any movement, as reaching for a ball and passing it from one hand to the other, he might work during his whole lifetime at that one puzzle before he would be likely to solve it by getting exactly the right muscles and in the right combinations.

The second theory, though contrary to ordinary observation and opinion, has many facts to support it. For example, it is well known that children and even adults who have never learned to swim sometimes succeed in swimming ashore when left in the water with nothing to do but sink. Fathers sometimes use this method of teaching their sons to swim. Many parents have noticed that their children learned to walk and run with surprising rapidity after they began. The most striking instance of this kind coming under my notice is thus described for me by the father, Supt. Hall, of North Adams, Mass.

"In reply to yours of March 25th, I give you the following account of how my little daughter Katherine learned to walk. She was the youngest of a family of five. The other children had learned to walk soon after they were a year old, and in the normal fashion by being encouraged to put forth a series of efforts until they were able to go alone. Katherine was a normal child in other respects, bright, active and healthy, yet unable to walk a step when she was seventeen months old. Of course, we were anxious, fearing that the cause of this inefficiency might be physical, especially as she persisted in crawling and absolutely refused to try to help herself under the encouragement of any assistance.

"At last we referred the matter to a physician, who said: 'It is a peculiar case, and I can hardly tell whether the difficulty is physical or mental. If there is no improvement in a short time, call me again.' Shortly afterwards I came home

one day at noon, and, placing my cuffs on a table in the sitting-room, threw myself on a lounge to rest. Katherine happened to notice the cuffs from where she sat on the floor, and, crawling across the room, pulled herself up by the leg of the table, and, reaching out with one hand while she held on to the table with the other, took a cuff off from the table and slipped it on over her wrist. Of course, to do this she had to stand alone. I noticed it at once, and was surprised when she reached out her other hand for the other cuff and slipped that on, and then stood looking in a very interested way at the cuffs on both wrists. Then, to our great surprise, she turned towards me with a very pleased expression on her face and walked as confidently and easily as any child could. Not only this, but she immediately ran across the room, through another room and around through the hall-way, not simply walking, but running as rapidly as a child four or five years of age would. What surprised us most was that she did not seem to be wearied by her effort at all.

"We allowed her to keep the cuffs on for ten minutes or more, and she was on her feet all the time. At last she sat down a moment, rested, and then, strange to say, got up on both feet without assistance and commenced to run around the room again. As an experiment, I took the cuffs off, and she was as unwilling to try to walk as before. We could not possibly induce her to take a single step without the cuffs. When, however, we allowed her to put them on, she seemed to be greatly delighted and walked and ran as before. The result was that I gave her an old pair of cuffs to put on and allowed her to wear them for two days. This was the only way we could keep her from crawling. After that time she seemed to be able to get along without the cuffs, and has not crawled any since."

Instances similar to this of sudden acquisition of control of the vocal organs are not unusual. Numerous cases of remarkable movements by somnambulists and by persons frightened or excited are so common that it is sometimes said that instinctive action is more perfect than deliberate action. The fact that such instances are rare, while most children seem to spend considerable time in learning movements, is not positive proof

that such movements as walking and swimming are not inherited movements. It may be claimed that as fast as the nervous and muscular systems develop the child begins making the movements which when combined with others constitute the movements of walking; but that those movements ordinarily looked upon as practice and regarded as the cause of nervous and muscular development are in reality merely the effect and sign of the hereditary perfectment of the nervous and muscular systems which is going on. Such movements as those of walking and swimming may, therefore, be wholly hereditary, but it seems reasonable to suppose that the development of those movements is hastened and in part produced by practice, and certainly it cannot be claimed that all the various movements of work and play which human beings perform are inherited rather than acquired, especially when it comes to the manipulation of tools.

The third theory is the one more commonly held by psychologists and physiologists, and in a general way is probably the most nearly correct, but in my judgment it needs to be modified in the direction of the last and made much more definite. Observation of young children has shown clearly that the infant inherits the power to make many reflex, instinctive, expressive and impulsive movements, and that these simpler movements are combined in performing the various voluntary movements which he afterwards performs. It seems to be the common opinion that chance and imitation are important factors in effecting such combinations, while some seem to think that the child learns the simpler movements and then by an act of constructive imagination combines them in the proper way to effect his purposes. Professor Baldwin, who has perhaps contributed more than any one else to the subject, has in part eliminated chance by showing that there is a tendency in every organism so to act as to continue, increase or repeat favorable stimuli. The performance, repetition and perfectment of a movement do not, therefore, depend entirely upon the chance production or repetition of the stimulus by the environment, but the tendency in the animal to the circular form of reaction causes the stimulus to be repeated again and again. He does not, however, make sufficiently clear

the physiological basis of this tendency, and he seems to allow too large an element of chance in the determination of the course of nervous impulses within the organism. When a child repeats again and again a sound, as children so often do in the third quarter of the first year, it must be because the auditory sensory center is in closer connection with the motor center for the vocal organs than with any other motor center; otherwise the limbs would be just as likely to move as the vocal organs. More than this, the sensory center for that sound must be more closely connected with the center for producing it, or else any other sound would be just as likely to be made. There are probably more than a score of muscles concerned in articulation, and only when just the right ones contract in just the right degree will a given sound be produced; hence the number of different combinations mathematically possible is hundreds of millions. It cannot, therefore, be a matter of chance when a child repeats, after a few trials, a sound that he has heard; but it must depend upon physiological structure that makes the path more open between certain auditory centers and corresponding motor speech centers. Again, when a child imitates a movement he sees, it must be because there is a connection between the visual sense center and the motor center for moving the part in a corresponding way. Of course, it is a familiar fact that there is an excess of motor energy set free in all attempts to make new movements, especially in the case of children, which causes many other than the necessary muscles to contract; but physiological openness of certain paths rather than chance determines which movements shall be selected for repetition.

The next point which I wish to emphasize is that there is an inherited physiological space relation between the visual stimulus of an object in a certain position and the muscles for moving to that object. A young chicken succeeds in picking up a grain of meal, not because he mentally judges the direction and distance, but because the visual sensation calls the right muscle into play. In a similar way, a child grasps an attractive object, not because he knows its direction and distance, but because the visual sensation calls the proper muscles into play. So accurate is this physiological relation between visual sensations and move-

ments that, though I experimented frequently from the time she began to grasp at about three months, I never succeeded in getting my little girl to try to grasp an object more than four or five inches beyond her reach, and rarely so far as that. She would stretch her hands towards more distant objects that she wanted, but not with the grasping movement. The direction of her movement was also from the first nearly as accurate as the fixation with her eyes. This physiological space relation of certain motor reactions to certain stimulations is, in my judgment, of an importance hitherto unappreciated in explaining not only the development of voluntary movements, but also in explaining ideas of space.

Close observation of the earliest attempts at grasping convinced me that the only element prominent in consciousness at first is the visual sensation of the object. After it has been reached, reflexively grasped when touched and instinctively brought to the mouth several times, disappointment is shown if the hand, instead of the object, comes in contact with the lips, showing that there was then expectation of a certain kind of sensation that was not realized. The young child in grasping objects has a sensation or image of the object in a certain position and an image of a sensation to be gotten; but according to my observations there is no evidence that his consciousness is concerned at all with the movements he is making in order to get hold of the object and bring it to him. The same is true of all the earlier voluntary movements of the child, and attention to the movement itself hinders rather than helps in learning the movement. In the case of Superintendent Hall's little girl there was inability to walk so long as she thought about her movements; but as soon as her attention was concentrated upon getting the cuffs on and carrying them around she succeeded perfectly, though she had never tried it before.

Every adult knows that if he thinks about how he is doing a thing he can do it much less perfectly than when he thinks merely about what he wants to do; yet it is a common belief that one in learning any act must go through a stage of quite acute consciousness of the movements involved. I maintain, on the contrary, that children do not ordinarily go through any

such stage in learning their earlier movements, and that it is not usually necessary for either children or adults to go through such a stage of consciousness of all, or even of a large proportion, of the elementary movements involved in the new act.

I have not time to give facts in support of this conclusion, nor to point out its importance in the solution of various educational problems ; but I will close with one or two general considerations. In the history of the race arts have always preceded sciences ; men have learned to do things, then reflected upon how they do them, analyzed to discover elements, then determined the general laws according to which the actions may be successfully performed, and this order of procedure is the natural one to the child. It is possible that in some cases short cuts may be taken, as Baldwin has suggested, and possibly the order may sometimes be reversed and time saved. A person who knows one language, for example, may possibly learn another language more quickly by studying its grammar first ; but I am sure that a child who knows no language could not learn one by beginning with the grammar. Adults who are able to make many movements may learn more quickly a new movement by having attention called to some of the elements, though probably never by having it called to all ; but a young child would be hindered rather than helped by such a process. This is true, not simply because of the general tendency of the mind to develop in this order, but because the past experience of the race has developed a very definite system of relations between various stimuli and various simple movements, and has probably developed less definitely various combinations of simple movements and a tendency to other combinations in the attainment of ends frequently striven for by the race. The teaching of a movement by having each of its elements learned, and then having these elements combined and used, is not only a reversal of the natural order in attaining an end and a misdirection of attention, but is an undoing of what has been partially done by the experience of our ancestors, instead of completing the process.

THE INSTINCTIVE REACTION OF YOUNG CHICKS.

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The data to be presented in this article were obtained in the course of a series of experiments conducted in connection with the psychological laboratory of Harvard University during the year '96-'97. About sixty chicks were used as subjects. In general their experiences were entirely under my control from birth. Where this was not true the conditions of their life previous to the experiments were known, and were such as would have had no influence in determining the quality of their reactions in the particular experiments to which they were subjected. It is not worth while to recount the means taken so to regulate the chick's environment that his experience along certain lines should be in its entirety known to the observer and that consequently his inherited abilities could be surely differentiated. The nature of the experiments will, in most cases, be such that little suspicion of the influence of education by experience will be possible. In the other cases I will mention the particular means then taken to prevent such influence.

Some of my first experiments were on color vision in chicks from 18 to 30 hours old, just old enough to move about readily and to be hungry. On backgrounds of white and black cardboard were pasted pieces of colored paper about 2 mm. square. On each background there were six of these pieces,—one each of yellow, red, orange, green, blue and black (on the white ground) or white (on the black). They were in a row about half an inch apart. The chicks had been in darkness for all but three or four hours of their life so far. During those few hours the incubator had been illuminated and the chicks had that much chance to learn color.

The eight chicks were put, one at a time, on the sheet of cardboard facing the colored spots. Count was kept of the number of times that they pecked at each spot and, of course, they were watched to see whether they would peck at all at random. In the experiments with the white background all the colors were reacted to (*i. e.*, pecked at) except black (but the letters on a newspaper were pecked at by the same chicks the same day). One of the chicks pecked at all five, one at four, three at three, one at two and one at yellow only. These differences are due probably to accidental position or movements. Taking the sums of the reactions to each color-spot we get the following table :

I.	Times reacted to.	Total number of pecks. ¹
Red,	12	31
Yellow,	9	21
Orange,	6	34
Green,	5	11
Blue,	1	3

I should attach no importance whatever to the quantitative estimate given in the table. The only fact of value so far is the evidence that from the first the chick reacts to all colors. In no case was there any random pecking at the white surface of the cardboard.

On a black background the same chicks reacted to all the colors.

II. is a table of the results.

II.	Times reacted to.	Total number of pecks.
White,	6	19
Blue,	4	11
Red,	4	8
Green,	4	4
Orange,	2	7
Yellow,	2	4

In other experiments chicks were tried with green spots on a red ground, red spots on a green ground, yellow spots on an orange ground, green spots on a blue ground, and black spots

¹This double rating is necessary because of the fact that the chick often gives several distinct pecks in a single reaction. The 'times reacted to' mean the number of different times that the chicks noticed the color.

on a white ground. All were reacted to. Thus, what is apparently a long and arduous task to the chick is heredity's gift to the chick. It is conceivable, though to me incredible, that what the chick reacts to is not the color, but the very minute elevation of the spot. My spots were made so that they were only the thickness of thin paper above pasteboard. Any one who cares to resort to the theory that this elevation caused the reaction can settle the case by using color-spots absolutely level with the surface.

INSTINCTIVE REACTIONS TO DISTANCE, DIRECTION, SIZE, ETC.

I have purposely chosen this awkward heading rather than the simple one Space-Perception, because I do not wish to imply that there is in the young chick such consciousness of space-facts as there is in human beings. All that will be shown here is that he reacts appropriately in the presence of space-facts, reacts in a fashion which would in the case of a man go with genuine perception of space.

If one puts a chick on top of a box in sight of his fellows below, the chick will regulate his conduct by the height of the box. To be definite, we may take the average chick of about 95 hours. If the height is less than 10 inches he will jump down as soon as you put him up. At 16 inches he will jump in from 5 seconds to 3 or 4 minutes. At 22 inches he will still jump down, but after more hesitation. At 27 $\frac{1}{2}$ inches 6 chicks out of eight at this age jumped within 5 minutes. At 39 inches the chick *will not jump down*. The numerical values given here would, of course, vary with the health, development, hunger and degree of lonesomeness of the chick. All that they are supposed to show is that at any given age the chick without experience of heights regulates his conduct rather accurately in accord with the space-fact of distance which confronts him. The chick does not peck at objects remote from him, does not, for instance, confuse a bird a score of feet away with a fly near by, or try to get the moon inside his bill. Moreover, he reacts in pecking with considerable accuracy at the very start. Lloyd Morgan has noted that in his very first efforts the chick often fails to see the object, though he hits it,

and on this ground has denied the perfection of the instinct. But, as a matter of fact, the pecking reaction may be as perfect at birth as it is after 10 or 12 days' experience. It certainly is not perfect then. I took nine chicks from 10 to 14 days old and placed them one at a time on a clear surface over which were scattered grains of cracked wheat (the food they had been eating in this same way for a week) and watched the accuracy of their pecking. Out of 214 objects pecked at 159 were seized, 55 were not. Out of the 159 that were seized, only 116 were seized on the first peck, 25 on the second, 16 on the third, and the remaining two on the fourth. Of the 55 that were not successfully seized, 31 were pecked at only once, 10 twice, 10 three times, 3 four times and 1 five times. I fancy one would find that adult fowls would show by no means a perfect record. So long as chicks with ten days' experience fail to seize on the first trial 45% of the time, it is hardly fair to argue against the perfection of the instinct on the ground of failures to seize during the first day.

The chick's practical appreciation of space facts is seen further in his attempts to escape when confined. Put chicks only twenty or thirty hours old in a box with walls three or four inches high and they will react to the perpendicularity of the confining walls by trying to jump over them. In fact, in the ways he moves, the directions he takes and the objects he reacts to, the chicken has prior to experience the power of appropriate reaction to colors and facts of all three dimensions.

INSTINCTIVE MUSCULAR COÖRDINATIONS.

In the acts already described we see fitting coördinations at work in the chick's reactions to space facts. A few more samples may be given. In jumping down from heights the chick does not walk off or fall off (save rarely), but jumps off. He meets the situation "loneliness on a small eminence" by walking around the edge and peering down; he meets the situation "sight of fellow chicks below" by (after an amount of hesitation varying roughly with the height) jumping off, holding his stubby wings out and keeping right side up. He lands on his feet almost every time and generally very cleverly. A four days' chick

will jump down a distance eight times his own height without hurting himself a bit. If one takes a chick two or three weeks old who has never had a chance to jump up or down, and puts him in a box with walls three times the height of the chick's back, he will find that the chick will jump, or rather fly, nearly, if not quite, over the wall, flapping his wings lustily and holding on to the edge with his neck while he clammers over. Chicks one day old will, in about 57 per cent. of the cases, balance themselves for five or six seconds when placed on a stiff perch. If eight or nine days old, they will, though never before on any perch or anything like one, balance perfectly for a minute or more. The muscular coördination required is invoked immediately when the chick feels the situation "feet on a perch." The *strength* is lacking in the first few days. From the fifth or sixth day on chicks are also able (their ability increases with age) to balance themselves on a slowly swinging perch.

Another complex coördination is seen in the somewhat remarkable instinct of swimming. Chicks only a day or two old will, if tossed into a pond, head straight for the shore and swim rapidly to it. It is impossible to compare their movements in so doing with those of ducklings, for the chick is agitated, paddles his feet very fast and swims to get out, not for swimming's sake. Dr. Bashford Dean, of Columbia University, has suggested to me that the movements may not be those of swimming, but only of running. At all events, they are utterly different from those of an adult fowl. In the case of the adult there is no vigorous instinct to strike out toward the shore. The hen may try to fly back into the boat if it is dropped overboard, and whether dropped in or slung in from the shore will float about aimlessly for a while and only very slowly reach the shore. The movements the chick makes do look to be such as trying to run in water might lead to, but it is hard to see why a hen shouldn't run to get out of cold water as well as a chick. If, on the other hand, the actions of the chick are due to a real swimming instinct, it is easy to see that, being unused, the instinct might wane as the animal grew up.

Such instinctive coördinations as these, together with the walking, running, preening of feathers, stretching out of leg

backwards, scratching the head, etc., noted by other observers, make the infant chick a very interesting contrast to the infant man. That the helplessness of the child is a sacrifice to plasticity, instability and consequent power to develop we all know; but one begins to realize how much of a sacrifice when one sees what twenty-one days of embryonic life do for the chick brain. And one cannot help wondering whether some of the space-perception we trace to experience, some of the coördinations which we attribute to a gradual development from random, accidentally caused movements may not be more or less definitely provided for by the child's inherited brain structure. Walking has been found to be instinctive; why not other things?

INSTINCTIVE EMOTIONAL REACTIONS.

The only experiments to which I wish to refer at length under this heading are some concerning the chick's instinctive fears. Before describing them it may be well to mention their general bearing on the results obtained by Spalding and Morgan. They corroborate Morgan's decision that no well-defined specific fears are present; that the fears of young chicks are of strange moving objects in general, shock in general, strange sounds in general. On the other hand, no such general disturbances of the chick's environment led to such well-marked reactions as Spalding described. And so when Morgan thinks that such behavior as Spalding witnessed on the part of the chick that heard the hawk's cry demands for its explanation nothing more than a general fear of strange sounds, my experiments do not allow me to agree with him. If Spalding really saw the conduct which he says the chick exhibited on the third day of its life in the presence of man, and later at the stimulus of the sight or sound of the hawk, there are specific reactions, for the running, crouching, silence, quivering, etc., that one gets by yelling, banging doors, tormenting a violin, throwing hats, bottles, or brushes at the chick is never anything like so pronounced and never lasts one-tenth as long as it did with Spalding's chicks. But as to the fear of man, Spalding must have been deluded. In the second, third and fourth days there is no such reaction to the sight of man as he thought he saw.

Miss Hattie E. Hunt, in the *American Journal of Psychology*, Vol. IX., No. 1, asserts that there is no instinctive fear of a cat. Morgan did not find such. I myself put chicks of 2, 5, 9 and 17 days (different individuals each time, 11 in all) in the presence of a cat. They showed no fear; went on eating as if nothing about. The cat was still, or only slowly moving. I further put a young kitten (eight inches long) in the pen with chicks. He felt of them with his paw, and walked around among them for five or ten minutes, yet they showed no fear (nor did he instinctively attack them). If, however, you let a cat jump at chicks in real earnest, they will not stay to be eaten, but will manifest fear—at least chicks three to four weeks old will. I did not try this experiment with a lot of chicks at different ages, because it seemed rather cruel and degrading to the experimenter. When in the case of the older chicks nature happened to make the experiment, it was hard to decide whether there was more violent fear of the jumping cat than there was when one threw a basket or foot-ball into the pen. There was not very much more.

We may now proceed to a brief recital of the facts shown by the experiments in so far as they are novel. It should be remembered throughout that in every case chicks of different ages were tested so as to demonstrate transitory instincts if such existed—*c. g.*, the presence of a fear of flame was tested with chicks 59 and 60, one day old, 30 and 32, two days old, 21 and 22, three days old, 23 and 24, seven days old, 27 and 29, nine days old, 16 and 19, eleven days old, and so on up to twenty days old chicks. By thus using different subjects at each trial one, of course, eliminates any influence of experience.

The first notable fact is that there develops in the first month a general fear of novel objects in motion. For four or five days there seems to be no such. You may throw a hat or slipper or shaving-mug at a chick of that age, and he will do no more than get out of the way of it. But a twenty-five days old chick will generally chirr, run and crouch for five or ten seconds. My records show this sort of thing beginning about the tenth day, but it is about ten days more before it is very marked. In general, also, the reaction is more pronounced if a lot of chicks are

together, and is then displayed earlier (only two at a time were taken in the experiments the results of which have just been quoted). Thus the reaction is to some degree a social performance, the presence of other chicks combining with the strange object to increase the vigor of the reaction. Chicks ordinarily scatter apart when they thus run from an object.

One witnesses a similar gradual growth of the fear of man (not as such probably, but merely as a large moving object). For four or five days you can jump at the chick, grab at it with your hands, etc., without disturbing it in the least. A chick twenty days old, however, although he has never been touched or approached by a man, and in some cases never seen one except as the daily bringer of food, and has never been in any way injured by any large moving object of any sort, will run from you if you try to catch him or even get very near him. There is, however, even then, nothing like the utter fear described by Spalding.

Up to thirty days there was no fear of a mocking bird into whose cage the chicks were put, no fear of a stuffed hawk or a stuffed owl (kept stationary). Chicks try to escape from water (even though warmed to the temperature of their bodies) from the very first. Up to forty days there appears no marked waning of the instinct. They did not show any emotional reaction to the flame produced by six candles stuck closely together. From the start they react instinctively to confinement, to loneliness, to bodily restraint, but their feeling in these cases would better be called discomfort than fear. From the 10th or 12th to the 20th day, and probably later and very possibly earlier, one notices in chicks a general avoidance of open places. Turn them out in your study and they will not go out into the middle of the room, but will cling to the edges, go under chairs, around table-legs and along the walls. One sees nothing of the sort up through the fourth day. Some experiments with feeding hive bees to the chicks are interesting in connection with the following statement by Lloyd Morgan: "One of my chicks, three or four days old, snapped up a hive bee and ran off with it. Then he dropped it, shook his head much and often, and wiped his bill repeatedly. I do not think he had been stung; *probably*

he tasted the poison." (*Int. to Comp. Psy.*, p. 86.) I fed seven bees apiece to three chicks from ten to twenty days old. *They ate them all greedily*, first mashing them down on the ground violently in a rather dextrous manner. Apparently this method of treatment is peculiar to the object. Chicks *three* days old did not eat the bees. Some pecked at them but none would snap them up, and when the bee approached they sometimes sounded the danger-note. Finally an account may be given of the reaction of chicks at different ages, up to twenty-six days, to loud sounds. These were the sounds made by clapping the hands, slamming a door, whistling sharply, banging a tin pan on the floor, mewing like a cat, playing a violin, thumping a coal-scuttle with a shovel, etc., etc. Two chicks were together in each experiment. Three-fourths of the times no effect was produced. On the other occasions there was some running or crouching or, at least, starting to run or crouch; but, as was said, nothing like what Spalding reports as the reaction to the 'cheep' of the hawk. It is interesting to notice that the two most emphatic reactions were to the imitation mew. One time a chick ran wildly, chirring, and then crouched and stayed still until I had counted 105. The other time a chick crouched and stayed still until I counted 40. But the other chick with them did not and in a dozen other cases the 'meaw' had no effect.

I think that the main interest of most of these experiments is the proof they afford that instinctive reactions are not necessarily definite, perfectly appropriate and unvarying responses to accurately sensed and, so to speak, estimated stimuli. The old notion that instinct was a God-given substitute for reason left us an unhappy legacy in the shape of the tendency to think of all inherited powers of reaction as definite particular acts invariably done in the presence of certain equally definite situations. Such an act as the spider's web-spinning might be a stock example. Of course, there are many such instinctive reactions in which a well-defined act follows a well-defined stimulus with the regularity and precision with which the needle approaches the magnet. But our experiments show that there are acts just as truly instinctive, depending in just the same way on inherited brain-structure, but characterized by being vague, irregular, and, to some extent, dissimilar reactions to vague, complex situations.

The same stimulus doesn't always produce just the same effect, doesn't produce precisely the same effect in all individuals. The chick's brain is evidently prepared in a general way to react more or less appropriately to certain stimuli, and these reactions are among the most important of its instincts or inherited functions. But yet one cannot take these and find them always and everywhere. This helps us further to realize the danger of supposing that in observation of animals you can depend on a rigid uniformity. One would never suppose because one boy twirled his thumb when asked a question that all boys of that age did. But naturalists have been ready to believe that because one young animal made a certain response to a certain stimulus, the thing was an instinct common to all in precisely that same form. But a loud sound may make one chick run, another crouch, another give the danger call, and another do nothing whatever.

In closing this article I may speak of one instinct which shows itself clearly from at least as early as the sixth day, which is preparatory to the duties of adult life and of no other use whatsoever. It is interesting in connection with the general matter of animal play. The phenomenon is as follows: The chicks are feeding quietly when suddenly two chicks rush at each other, face each other a moment and then go about their business. This thing keeps up and grows into the ordinary combat of roosters. It is rather a puzzle on any theory that an instinct needed so late should begin to develop so early.

DISCUSSIONS.

PROFESSOR MÜNSTERBERG ON MYSTICISM.

The criticism of 'The New Psychology,' it seems, has a sequel. We have an equal and presumably impartial attack upon Mysticism, of which one form is psychical research. In taking up the cudgels, however, I am not going to defend this curious department of inquiry. Even among those who are interested in it there is room enough for scepticism of the most scrutinizing sort. I accord any man whatever opinion he pleases to have about it. But I should ask that the scientific method that Professor Münsterberg demands in this and all psychological work be represented in his criticism, or a frank admission made that dogmatism is the fundamental instrument of knowledge. To me his recent article in the *Atlantic Monthly* is one of the most amusing documents that I have ever had the pleasure of reading. I am not going to attack the discussion as a whole, but only to deal with that part of it which criticises psychical research. Let us see how much science there is in his method.

Professor Münsterberg in one passage confesses that until the last summer vacation he felt rather guilty for forming and stating opinions on this subject before reading its literature. He then proceeds to enjoy his vacation 'in working through more than a hundred volumes of the so-called evidence.'!!! Just think of that! A scientist spending the summer rest of a few weeks reading more than one hundred volumes of matters involving a question of evidence, and actually forming what he thinks a scientific conclusion on them!! I do not believe there are twenty-five volumes in existence on this subject that any sane man ought to read at all, let alone doing it at such a time. I have watched this subject for ten years, and have in all that time read no more than ten volumes, some of them exceedingly carefully, and I did not dream of forming an opinion or irreversable conclusion upon them. On this subject of psychical research, unless you have made sufficiently decisive experiments personally (not merely curing one hallucination by suggestion), it may take a hundred years to arrive at any scientific conclusion at all. But would Professor Münsterberg advise his students to study psychology generally at the rate of 'more

than a hundred volumes' a vacation when the temperature is between eighty and a hundred? Moreover, what right has a professed scientist to depend upon books, no matter how many of them, for a conclusion that involves matters of very delicate experiment, and not analytical and introspective methods? Professor Münsterberg says that he is not a detective. He should then not pronounce upon problems that require that sort of ability. Here is a place for a confession of ignorance and to eschew the pretensions of knowledge.

Apropos of this last remark it is well to recall another singular confession of our author. His reason for not making a personal investigation into this question is that it is not 'dignified to visit such performances' as séances!! If physiology and biology had acted on this maxim we should have known very little about life on the one hand, and about brain processes on the other, on which Professor Münsterberg relies so much for his assurance against mysticism. Dignity is not anything that should stand in the way of experiment or exact method. I confess I admire Darwin for playing a bassoon to his garden plants to test some supposition, though his neighbors, had they seen him at it, would have thought him suitable for a lunatic asylum. Science at one time was too dignified to examine the stories about falling meteors, but it came to terms at last. It did the same with hypnotism. It first packed a jury to condemn it, and thought it had laid the monster, but after forty years contempt decided to embrace it as a fact nevertheless!! Its dignity would not save its scepticism.

It seems, again, that Professor Münsterberg cannot protect himself against fraud. He thinks the scientist is trained to 'an instinctive confidence in his coöperators.' Granted. All scientific truth involving the coöperation of others, then, must be taken on authority. Everything depends upon the assurance of men that there is no fraud who have either not looked for it or are not able to detect it!! When science comes to that pass I shall have done with it. A man who cannot protect himself against fraud must not expect his opinion to be worth very much. He may read 'more than a hundred volumes' in his vacation and form theories in that way, but he must not expect us to take his experimental work seriously.

Let us have some science. "If I talk with others whom I wish to convince there is no physical process in question, mind reaches mind, thought reaches thought, but in this aspect thoughts are not psychophysical phenomena in space and time, but attitudes and propositions in the sphere of the will." Well, this is either telepathy with a vengeance or it is blank nonsense. Just think of the statement that

there is no physical process in the communication of thoughts!! Where is the evidence for all this? Can science escape the demand for fact to prove an assertion? What facts has Professor Münsterberg to show that this view is either true or intelligible? Then, what does he mean by a 'proposition in the sphere of the will'? While we are playing 'ducks and drakes' with the language of science, why not go further and say that fear is a feeling in the sphere of logic? As to what Professor Münsterberg may intend by this description of the communication of ideas, I can well imagine. But I can do it only by having some knowledge of the process myself, and not from any statement that he makes. When I wish to transmit my thought to others by talking I make a disturbance in the air, and the receiver interprets the sound. Now, if 'communication' be convertible with 'interpretation' we may agree that there 'is no physical process in question,' but in all intelligible parlance, outside the suppositions of telepathy, 'communication' means that the physical process is a part of the totality. Otherwise there is no interpretation even, and the only resource for common thoughts would be universal telepathy, which Professor Münsterberg will not admit as possible. And neither for nor against one or the other of the claims does he produce any facts!! It is simply bald blank assertion, and this is supposed to be science after laughing at the dogmatism of the Middle Ages!

We have another illustration of the same sort of thing. "The ethical belief in immortality means that as subjects of will we are immortal; that is, that we are not reached by death. For the philosophical mind, which sees the difference between reality and psychological transformation, immortality is certain; for him the denial of immortality would be even quite meaningless. Death is a biological phenomenon in the world of objects in time; how, then, can death reach a reality which is not an object, but an attitude, and, therefore, neither in time nor space? Our real inner subjective life has its felt validity, not in time, but beyond time: it is eternal." This is science, I suppose!! Not a fact to prove it. It is said that 'philosophy' shows this. Whose philosophy? On what facts is it founded? Then, again, *what* is immortal? We are not told what it is. From a previous reference to the 'ethical belief' Professor Münsterberg says that it ends in mysticism, and I imagine that what he says of it here is intended to be condemned as compared with the philosophic verdict. If so it cannot be the subject of will, and if it is not this we have immortality affirmed without telling us what is immortal. But assuming it is 'we as subjects of will' that are immortal, what is this 'we,'

especially when an earlier passage asserts that the 'inner reality,' which is here said to be eternal, 'never consists of psychological phenomena.' But this sort of criticism aside as savoring of quibbles, I must press the scientific demand for fact to show that the tremendous assertion here made has another basis than the mere speculative opinion of the author. As for myself I must contend that there is not one iota of rational evidence for immortality, of any intelligible or desirable kind, outside the sphere and method of psychical research. I do not maintain that even this is rational, but it is all that can lay the slightest claim to being rational from the standpoint of science, and the philosophic standpoint I absolutely reject as merely a process of looking into one's navel to solve the riddle of the Sphinx. Heaven knows that the spiritualist's 'scientific' evidence for his belief has been meager and poor enough, but the philosopher's has been worse. It has rested mainly on 'dignity' and 'dignified' methods, tempered with equivocation and hypocrisy to escape persecution. I follow the method and accept the verdict of science on this matter. If it gives me trustworthy facts making immortality a rational belief, I can affirm it; if it cannot produce these facts I either suspend judgment or accept the probability, from the connection of consciousness with a perishable organism, that this function dissolves with it. And when I speak of immortality I mean personal survival; that is, the continuance of consciousness beyond the life of the body. Any other immortality I do not care a picayune for, and would not be caught juggling with any affirmative proposition containing the term. What amazes me is that any man making the slightest pretense to scientific method would, after the terrible lesson of scholasticism, attempt for a moment to make such a tremendous assertion as that of immortality without at least a small array of empirical facts to support it. There is another very singular passage. After telling us in fine language that science must not prejudge a question, must not 'reject a fact because it does not fit into the scientific system of to-day,' etc., Professor Münsterberg goes on to say: "This is the old text," etc., "Yet it is wrong and dangerous from beginning to end, and has endlessly more harm in it than a superficial view reveals, as it is in last consequences not only the death of real science, but worse, the death of real idealism." Well, we have to choose between psychical research and idealism. But what is idealism? Is that so clear in these times that men have no freedom to question it? When I read a book or essay on idealism I am reminded of the sermon which the old woman could not understand, but which, nevertheless, edified and consoled her by

the presence in it of 'the blessed word Mesopotamia.' Idealism is unintelligible, but then it is the basis of ethics and art! I say frankly that if I had to choose between psychical research and idealism I should unhesitatingly take the side of psychical research for clearness and knowing 'where you are at.' For I do not know any field of thought which is more full of intellectual hobgoblins than that of Kanto-Hegelian idealism. I am not opposing idealism, because if I am allowed to define it for myself I should say that it is a mere truism. It is to me like the proposition that water is wet or blue is a color. But I do not expect to solve any problems with it. Least of all, do I consider it a sanctuary in which I am not allowed to say anything about either materialism or spiritualism. The only way that idealism can get into antagonism with any theory is to limit itself to solipsism. In any other form it is only a field for that kind of intellectual gymnastics which, as Kant remarks, characterizes the heroes of Valhalla. They are forever hewing down shadows which only spring up again to renew their ceaseless and bloodless conflict.

But the most remarkable thing about this passage is its distinct renunciation of scientific method for a dogmatism that knows all about the universe without any further inquiry. I do not see why a man talks any more about 'science' and 'scientific' method as an enemy of superstition when he shows that he has no other conception of it than that which denies the right to revise existing opinion. Evidently, science and dogmatism are the same here, while idealism is 'that blessed word' which is to exorcise all spirits except its own, and they are as shadowy as the ghosts that inhabit Homer's Cimmerian shades.

Taking the article as a whole, I do not see why Professor Münsterberg did not distinguish between the relevancy of the various alleged phenomena that he was criticising. Table turning, telepathy, clairvoyance, hypnotism and what not were lumped together with no more conception of their differences than is usually displayed by the spiritualist himself. The fact is that not one of them, unless we except telepathy, even if they were genuine, has any bearing on the question of spiritualism, and telepathy, if true, might be used as a very effective bar to spiritualism. But as in the phenomena of insanity and hallucinations, which, by the way, Professor Münsterberg is not too dignified to study, we can classify alleged facts and discuss their relevancy to the hypotheses which they are said to support. Professor Münsterberg should have read that hundred volumes with sufficient care to discover the distinction that a scientist ought to master at first. There is no use to assume that the spiritualist has the right conception

either of his problems or of his facts. I consider that he has neither, as a rule, and it would save some reputation if these alleged phenomena could be treated as patiently as are those of insanity. I am here defending only the method of psychical research. I do not care what becomes of its facts or alleged phenomena. I merely ask that its critics deal with it from the inside, and not in a confessedly *a priori* manner. My attention to it for ten years has convinced me that there is enough in the subject to engage serious consideration, no matter what the conclusions may be. In fact, the plausibility of some tremendous claims is so great, and so thoroughly in accord with what the common mind in this sceptical age would like to see established, that it will require all the severity and sceptical scrutiny of scientific method at our command to get any proper attention to normal psychology. I happen to know some genuine supernormal phenomena, not explainable by either fraud, illusion, or suggestion, and whose significance, or at least plausible significance, will have to be reckoned with by men who, like the mediæval theologians refusing to look through Galileo's telescope, cannot sacrifice their dignity for the sake of controlling a movement instead of following in its wake. Some of those who are making haste to laugh at it without studying it and its alleged facts at first hand will find themselves where they will have either to lose their influence for all psychology or, in order to save it, will have to 'eat crow,' and 'white crow' at that. It is not the remarkable nature of the alleged phenomena of psychical research that gives them so much interest and influence; for the scientific scepticism of the last century has very well fortified the average intelligence against some of the vagaries of spiritualism. But it is the wonderful triumphs of invention and discovery in the fields of both science and art that have destroyed the ordinary criteria of the limits of human knowledge and capacity, so that the average mind is rapidly coming to expect that almost anything is possible. Electricity, the telephone, Roentgen rays, the phonograph, surgery, hypnotism, etc., have opened up such a fairy land of wonders and possibilities to the common mind that it is not surprising to see many otherwise balanced intellects yielding to the claims of spiritualism. Science must reckon with this condition of mind and, instead of employing dogmatism against it, treat its alleged phenomena in the same serious and sympathetic manner that insanity receives. Science has taught us not to burn witches, as they did once, but to put them in asylums. Perhaps the same generous treatment of psychical research may still further extend the operations of humanity. To do this also it will not re-

quire us to spend our summer vacations in reading any very large amount of occult lore.

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MR. MARSHALL AND THE THEORY OF RELIGION.

Perhaps the most interesting problem with which evolutionary science has to deal is as to the social function of religion. Religion as a very general and large phenomenon could have survived and grown only as a useful element in the struggle of existence of the individual and his society. This evolutionary assumption that what is, subsists and increases only by virtue of function; that natural evolution is an evolution of utilities, and that useless factors are always speedily eliminated in the struggle of existence, is really a close approach to the old doctrine of evidences by which the theologian makes the warrant of religion to be the function which it plays in man's life. For instance, the apologist for prayer has always assured us that such a practice could not have arisen and developed except that it met a real need of human life and was in some way truly answered, and the evolutionist as biologist and sociologist likewise finds that prayer by its very existence shows its validation as an important factor in human life, if not in the way the religionist assumes, at least in some way. It is an interesting fact that, though science, by widening the domain of naturalism indefinitely, has shown that the religions are ineffectual in their methods, yet science, by its own assumption, sees in religion a function which has arisen in the struggle of existence.

Of recent interpretations of religion from the point of view of evolutionary science Mr. H. R. Marshall's 'Instinct and Reason' is the most notable and thoroughgoing. Mr. Marshall finds that evolution is toward organism, which is action of the part for the whole, and instinct is the psychic side of this organic tendency, while reason is the correspondent of individualistic action. The main stress of evolution is to subordinate the individual organ to the organic whole, the eye to minister to the body rather than to itself, the individual body to minister to the perpetuation of the species and of social wholes. But individualism is also a primitive and strong interfering tendency, and so, to give weight to the organic, Nature gives birth to religion as an instinct restraining us from undue individualism. Thus, when inclined to selfish actions religion appears as restraint, and so impulsive to social activity. The earliest sociality has to do with the perpetuation of the

species and the family relations; hence the earliest form of religion is phallicism. Religion is in this and all its later forms fundamentally a 'governing instinct' as suppressing individualism and helping to sociality. This is its utility amidst all its seemingly strange and perverse forms, an eminent utility which is subserved by all expressions of the religious instinct, fasting, prayer, sacrifice, etc.

That social utility is the function whereby religion has persisted and increased in human affairs is a familiar thought, but the merit of Mr. Marshall is that he has given this a large biological setting, and has brought it into line with the evolutionism of to-day. In his theory religion loses its absolutism as worship of the Deity. Religion is "the restraint of individualistic impulses to racial ones," implying that the belief in the Deity as usually found being from the psychological point of view an attachment to, rather than of the essence of, the religious feeling. The conservatism of religion means merely that it is instinct, which is by its nature conservative, and thus religion opposes individualism as an action variant from the general racial forms. Religion appears as restraining influence, an instinctive '*do not*,' even in its most egotistic forms, as it marks a dependence and a certain outward reference of conduct. Thus religion has its value, not in its supposed intrinsic advantage of obtaining good things from a deity, which is mostly fiction and illusion, but as repressive to the lower selfish instincts which tend to make man an independent unsocial being.

Now, in touching upon this theory, we must first remark that it reposes in a large part upon a one-sided view of organism. Mere natural organism is in its origin and early stage not an altruism of part to whole; it is in the struggle of existence a method of advantage by a reciprocity of individuals; a mode of exchange of values whereby the exchanger always seeks to give the least for the most; to get off with as small a *quid pro quo* as possible, or none at all, if the individual is strong enough. The struggle for advantage in organic reciprocity is common to the origin and early progress of organism and of altruism as mere justice. In the crude struggle of existence organic social forms arise and develop to a certain degree of community of interest and reciprocity, but with an intrinsic struggle within the organism itself for dominance by each organ. Societies are very largely of this type even in civilized life, as in the keen rivalry of industrialism and commercialism, which takes every advantage for a bargain. And in every organism under purely natural conditions there is internal disharmony and rivalry as keen in its way and as selfish as the struggle of the individual society with other societies. Hence religion in the sense of

restraint does not appear in pure naturalism, and is not essential to organic activity. The only restraint in elementary societies is weakness; the individual does not take more because he dares not. The social organism as range of reciprocity is at first wholly governed in the measure of reciprocity by force and cunning, and thus religion as restraint cannot be accounted coextensive with organism.

Now, in the primitive social *status*, where a mere competitive reciprocity is the mainspring, religion does in a very real sense exist as direct function as contrasted with the indirect function of restraint. Religion is primarily a method of reciprocity with superiors, a method involved in the struggle of existence, a method of worship, homage, devotion, etc., to find favor and obtain advantage with superiors, human and extra-human. Religion is a mode of socialization, a tie which binds child to parent, wife to husband, vassal to lord, as well as a tie to other superiors (supposedly existing in our view) as ancestors and nature deities. In militarism the direct function of religion is very great, and most very successful military leaders have been largely successful by being able to make themselves worshipped and adored by their men, and so securing perfect obedience, and unity, and dependence. And the leader surrounds himself with godhood by his special relationship to the ancestral and nature deities. It is in this way that what appear to us fictitious deities exercise a real and valuable utility in socialization; that is, by association with the living leader and chief. At the same time it must be recognized that mere religiousness as dependence upon the extra-human superior has been disadvantageous as destroying intelligent self-reliance. The superior and intense religiousness of the Hebrews never made them a great conquering nation. In the military competitive struggle of tribes for existence in West Africa, Miss Kingsley notes that the Fans, a comparatively unreligious tribe, are in the ascendant. In modern times the most successful militarism is not the most religious, but the most scientifically self-reliant. And the tendency is to rely less and less on religious observance, as noting of omens, invoking deities, etc., and to give the time to drill and tactics, and to make the soldier self-reliant in every emergency.

But undoubtedly religion as direct function plays yet a most important function in militarism and in political and social aggregation. A worship and homage bind the masses to the Czar Alexander and to Queen Victoria. Human beings transcendently exalted by their power, intellect, wealth, still enlist a vast amount of religious feeling and activity toward themselves, which unifies society under their absolute leadership.

But the tendency of modern socialization is not a unification of inferiors to supreme superiors, but of equals to equals in democracy. Hence, as the supremely superior is lost, religion as direct social function is lost also. But this form of religion has a survival form in the phrases of courtesy, such as, "I pray you and beg you to accept," by which by politeness we put ourselves as suppliants, but this is a mere ghost of the historic reality. Democracy emphasizes *vox populi* as *vox dei*; hence a positivist religion, religion as worship of humanity, is its natural outcome and its natural binding tie. And this religion directly emphasizes the true organic dependence of the individual and the supremacy of the race as such. But this evolution has scarcely begun, and it throws no light on the historic function of religion, and it can hardly be claimed that the direct function of religion sufficiently accounts for the large and important place of religion in the history of society, but it is still a valuable clue, and one much neglected by Mr. Marshall. Wherever immense superiority has appeared man has sought to ingratiate himself by acts of worship and homage, and this has been, and still is, a successful method and a social tie within the range of living human superiors; but it has been utterly insufficient when applied to what science deems fictitious superiors in environment, as ghosts and nature deities; nor can the religion of these have its full function as merely a background and basis for the living human superior fully to exalt himself and secure worship. Superior though nature be, we now know that the only real adaptation to it is not by the personal method of religion, but by intelligent self-reliant method of applied science. A vast deal of historic religion has thus failed of direct utility, and we can only suppose that the justification of its existence lies in some indirect function.

But, if as mere sanction and basis of authority of living human superiority this religion is scarcely sufficient function, we may add other indirect functions; for instance, dependence for example and also as restraint.

First, then, it may be said that religion as dependence and obedience thereby emphasizes and encourages a habit which is most necessary to socialization. We must consider this function of religion as of value in the history of society, and yet we must regard it as of no high significance. And we must note that religion begins rather as interdependence and reciprocity, the god being as dependent on the worshipper as the worshipper on him. In this matter early religion but reflects early society. But religion gradually loses reciprocity and becomes absolute dependence of man on deity, and at the same time

society assumes forms of absolute dependence on supreme power of monarch and despot and hierarch. It must, then, be considered likely not merely that religion sets a model for social relations, but *vice versa* also. Indeed, so far as religion concerns itself with the extra-human it is probably derived in its forms and spirit from religion as direct function in sociality. And, at any rate, religion as setting an example of absolute dependence is harmful to high socialization. Religion as fostering mendicancy, poverty, and all forms of unreciprocal dependence has been a distinct drag on social progress, which demands a high interdependence. The dependent classes are the problem of modern society. A vigorous independence and individuality is most valuable in societies where freedom reigns, where initiative has the freest scope, and progress is least hindered by conservative religion. The evolution of society is from a bare competitive reciprocity up through absolutism to the higher reciprocity of rational free individualism. Modern society is dominated by the scientific spirit, which demands that man work out his own salvation by practical appreciation of scientific knowledge and method. Science encourages an intelligent dependence on the specialist, but recognizes infallibility nowhere, and it must regard religion as anti-social so far as it emphasizes dependence on extra-human beings, and thus defeats real social coöperation to secure the end. Thus the highest sociality is a very complex interdependent, self-reliant specialism, which seeks to control nature through knowledge by natural means. Thus the British government repressing the plague in India is a higher, more successful type of society than the Hindu attitude and method toward the plague. And so everywhere religious socialization tends to be supplanted by scientific, and so far as religion hinders, by example or precept, it must be accounted as loss, as pathological rather than really functional.

We have mentioned the direct function of religion and one indirect function, namely as setting example and giving emphasis to dependence, and thus helping the bond between inferior and superior in society. These forms of function have little if any notice from Mr. Marshall, but the sociologist must, I think, regard them as very important, although hardly solving the whole problem of the function of religion. However, the negative side of the indirect function we have mentioned is restraint. That is, it is plain that religion, by encouraging dependence, thereby restrains and inhibits independence. In the moment of individualistic action we receive an instinctive check and an impulse towards social and racial activity. Religion thus holds us to the narrow path of typical racial action.

In regarding religion as merely negative and indirect function in repressing the lower, unorganic, individualistic instincts, Mr. Marshall must highly offend religionists in general, who make religion the absolute and supreme end of life, and not a social means. Religion has always had to do battle with the State, which has ever sought to enslave her and make her a tool, and the restraint function theory lies along the same line. But we note that so soon as the feeling permeates religion itself that God and the God-consciousness are mere social instruments religion is decadent, becoming formal and losing real vitality. That is, when indirect function becomes direct, and religion is observed for its social values, it loses its real power. It is certain that religion becoming conscious that it is not attaining its supposed and natural end, but seeking to continue itself as a mere social function, soon loses this value. However, in my paper on the psychology of religion (*PSYCHOLOGICAL REVIEW*, May, 1898) I have discussed this point of the socialistic theory, and it is only necessary here to remark the paradox that religion must be kept in ignorance of itself if it is to be itself and exercise its due function.

That restraint, the negative indirect function, as impulse which keeps us from offending the social order, counts largely in explaining the persistence of religion will at once be granted; but it cannot be regarded as the sole social utility of religion, as we have just sought to show. But early religion is mainly positive in function, and it seems highly improbable that it originated as negative inhibitive instinct. Religion primarily is a direct mode of obtaining advantage from *high superiors*, and has thus been carried on by successive generations until it has become instinctive, as, for instance, in prayer. It is not unlikely that prayer originated in the field of battle, when a fallen wounded foe prayed for his life to his conqueror. Religion arises, like all other utilities, as activity hit upon in a critical moment, in this case by some inferior in relation to superior, and then continued and improved and ultimately embodied in the race as instinct. This is the assumption which brings religion into the line of evolution. A Gold Coast negro prays, "God give me to-day rice and yams, gold and agries; give me slaves, riches and health, and that I may be brisk and swift!" (Taylor, *Primitive Culture*, Vol. II., p. 367.) Where is restraint implied in such religion? And yet it is an extremely common form in all degrees of culture, especially the lower. We cannot see trace of restraint-function in a vast mass of religion which must be accounted for. It is mainly as conducing toward a human intermediary as religious specialist, the sorcerer, and so towards a religious organization of society,

that such prayer and such religion in general have social bearing and utility, and partly as fostering the habit of dependance; but the restraint function cannot be said to appear at this stage. The simple, self-seeking one seeks without restraint a gratification by religious means. In a vast deal of similar religion there is evidently no conflict of racial and individual, and hence no higher governing instinct, as religion, to enter in and restrain the powerful instinctive, selfish activity. When religion becomes not merely personal means, but personal indulgence, religion becomes unrestraint, as we see, for instance, in many phenomena of revivalism. (See, for instance, Sidis, *Psychology of Suggestion*, *passim*.) Religion has done much to retard society by its formal conservatism, and to break up society in the heat of powerful emotions; and all this must be taken into account in a full view of the subject. The reformer and radical believe they have the God-given message and methods, and thus society has often been disturbed and sometimes even the family set at nought, as in hermitage and celibacy. In Christianity itself the family is secondary, and the disciple must be ready to hate even father and mother. Religion of the highest type, as interfering with the natural evolution of worldly success and advantage by individuals and societies, and setting up an unworldly, mystic, spiritual kingdom, destroys natural evolution, and hence naturalistic science must consider it pathological or seek some indirect function, as does Mr. Marshall.

Mr. Marshall's definition of religion as an instinctive check to individualistic action, a '*stop*,' '*do not*,' coming as from a high divine source, is far too narrow for either psychology or sociology, which must study religion as a general relation of inferior to superior in manifold forms and functions, as direct, and sanction, as example, as restraint. Every hypothesis, such as Marshall's, however skillfully deduced from biological assumptions, must be tested without bias by definite and extended study of historic facts, an immense and very complex field. And as Mr. Marshall fails to do this the scientific mind is disappointed. If even the religious experience of some single individual were thoroughly analyzed, something would have been gained for scientific exactness, but, as it is, his work remains as at best an ingenious suggestive speculation.

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PSYCHOLOGICAL LITERATURE.

The Psychology of Peoples. GUSTAVE LE BON. New York, The Macmillan Company. 1898. Pp. xx + 236.

As the title implies, the author believes that it is the psychology, chiefly the character, of peoples which determine their arts, institutions and history. The problems involved have been treated more fully in works on the civilizations of the East, and "this short volume may be regarded as a brief synthesis." "Each of the chapters composing it should be regarded as the conclusion arrived at by anterior investigations." The work is divided into five books dealing with the psychological characteristics of races, how these are displayed in the various elements of the culture of races, race-history determined by race-character, how psychological characteristics are modified, and the dissociation of race-character and decadence.

The central idea of the work is that races possess souls the acquisition of which marks the apogee of their greatness and the loss of which marks their decay. In this soul, sentiment, beliefs and interests are the moving and directing principles, and these constitute the basis of what the author calls character. Very meagre importance is assigned to the rôle of intelligence in the civilization of peoples, even beliefs being determined by suggestion and imitation so far as they affect the masses. Culture is merely a matter of memory: it can be acquired by inferior races, but does not affect character. Inferior races are distinguished from superior ones by differences of character solely; superior races are distinguished from each other by variations of both character and intelligence. Wide variations between individuals are the mark of developed races, but they do not count in determining the rank of a race. In estimating character, the masses alone are to be taken into account.

Ideas "do not exert an influence until, after a very slow evolution, they have been transformed into sentiments and have come, in consequence, to form part of character. They are then unaffected by argument and take a long time to disappear." "Religious ideas are among the most important of the guiding ideas of a civilization. The majority of historical events have been due indirectly to the variation of

religious beliefs. The history of humanity has always run parallel to that of its gods" (p. 235). "In religion, as in politics, success always goes to those who believe, never to those who are sceptical, and if at the present day it would seem as if the future belonged to the socialists, in spite of the dangerous absurdity of their dogmas, the reason is that they are now the only party possessing real convictions" (p. 178). "Faith is the only serious enemy which faith has to fear." "A people is only led by those who embody its dreams." The author gives an interesting account of the genesis of such faiths, a process in which reason plays but an insignificant, and suggestion an all-important, rôle. Propagation of faith is never by argument, and always by assertion, affirmation, impression. In time "the mere effect of imitation, acting as a contagion, a faculty with which men are generally endowed in as high a degree as are the big anthropoid apes," insures the spread of the idea; and then it is that it becomes a matter of sentiment and an element in character in the race. Then it is irresistible to argument. It is such factors of the common life of individuals which make the race 'a permanent being that is independent of time.'

The author holds that religious faith is the all-important moment in the life of humanity, but he considers the objects of religious faith to be 'illusions,' 'chimeras,' 'hallucinations' and 'the children of our dreams,' leaving the reader to infer that humanity is self-deluded. And this inference accords in general with the almost cynical and pessimistic tone which the author's thought at times assumes. He predicts that Europe will be swallowed up of socialism, and that America will be torn to pieces by an inter-race war between the incompatible elements that constitute her population. As to human nature in general, "of all the factors in the development of civilizations, illusions are, perhaps, the most powerful" (p. 207). The triumph of ideas "is insured when they are defended by the hallucinated and by enthusiasts. It is of slight importance whether they be true or not" (p. 206). "Doubtless it is man who created the gods, but after having created them he promptly became their slave" (p. 192). Is it cynicism, or is it simply a love for antithesis and epigram?

The work gives an interesting account of many of the facts of the race-consciousness and laws of its modifications. The style is interesting and strong. Many valuable suggestions are contained in the work. The author's personal philosophy of religion is, however, irrelevant to the theme, and rather weakens than strengthens the closing chapters of the work.

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Les origines de la psychologie contemporaine. D. MERCIER.
Louvain, Institut Supérieure de Philosophie, 1897. Pp. xii + 486.
Fr. 5.

This book is mainly a critical review of modern idealism from the standpoint of the neo-scholastic. To him the development and outcome of post-Cartesian idealism amounts to a *reductio ad absurdum* of its fundamental assumption. This assumption is none other than the sharp distinction introduced by Descartes between the two substances soul and body, soul essentially thought, body essentially extension, the two being utterly irreducible and incomparable, and their union a sphinx's riddle. Occasionalism, ontologism, and parallelism were but so many artificial efforts to guess the riddle. More consequent is the negative attitude of idealism, beginning with Locke's denial of *clear* ideas of the substances matter and mind, and passing through Berkeley's utter rejection of matter, and Hume's utter rejection of mind, to Kant's demonstration that knowledge of substance was not only unattained, but from the nature of thought unattainable. Kant still believed in empirical certainty, but here the author finds him inconsequent. If the objects of thought are determined by thought, no certainty is possible. The system of Hegel did not escape the subjectivistic difficulty, and was, indeed, a construction of the imagination rather than of reason (239). The truly logical consequences of the Kantian principles are seen in the doctrines of some recent French writers—such as Remacle, who contends that agnostic idealism must be extended to cover ideas themselves; that is, that even inner experiences, as known, are not the experiences as they are in themselves—or such as Louis Weber, who concludes that the only truth is the truth of a judgment, that outside of the judgment there is no existence.

This position of utter negation, though the only logical outcome of idealism, is self-destructive (340). For a judgment is meaningless unless it refers to some existence independent of the judgment. And unless an object is known as it is in itself, it evidently cannot be known to be different from our idea of it. The Kantian difficulty is the Cartesian folly, that of first conceiving a mind apart from its objects ('pure' reason), and objects apart from any mind (things 'in themselves'), and then wondering how the two are to be brought together (344).

As idealism proceeds from Descartes' definition of the soul, so from his definition of the physical has grown the conception of a universal reign of mechanical law. If all other bodies are machines, why not the human body; and if the single human body, why not those larger

organisms, the species in its development (Darwin), and human society in its history (Comte)? The system of Comte is thus on its positive side a carrying-out of the Cartesian doctrine of body, while on its negative, anti-metaphysical side it is an attempt to escape the more brutal materialistic consequences of the same doctrine (77).

Thus our author traces back to Descartes the two negative characteristics of contemporary psychology; its idealistic dependence, in all matters of theory, on the data of consciousness alone, and its positivistic neglect of ontology and rational psychology. Its third great characteristic, derived from natural science, is its increasing use of experiment. The author brings together evidence of the neglect of metaphysics in the universities and of the vigor of experimental study.

Among contemporary psychologists, Mercier picks out three as attempting either to harmonize or to supplement the current Cartesian tendencies. *Spencer* attempts a synthesis of the various conflicting elements. But he supplies no true organic unity. Nor does his 'transfigured realism' amount to more than a hazy belief. As for his doctrine of universal evolution, it is a mere expansion by analogy of a biological hypothesis, and owes its prestige less to agreement with facts than to its hold on the imagination (144, 145). *Fouillée*, though idealist and positivist, tries to avoid some of the negative consequences of these doctrines by introducing the conception of 'idees-forces,' *i. e.*, of the idea as dynamic, and of conscious or sub-conscious life as the dynamic principle of all physical existence. But when he would furnish a ground for knowledge of substantial reality, he can do no better than to allow the dynamic idea to create or postulate the reality it wants ('*fiat Deus*'). *Wundt* would enrich idealism by substituting the conception of 'actuality' for the conception of substance, and voluntarism for intellectualism. The latter attempt he carries too far. His genesis of ideas from the action of 'pure will' is as much a creation '*ex nihilo*' as the intellectualist's derivation of the will from mere ideas (214). Yet Wundt is not far from the kingdom. If he "could disencumber himself of his idealistic and positivistic prejudices, and of the false notion of substance that he borrowed from Kant, and follow freely the direction which his own researches force upon him, he would logically be led to accept the fundamental theories of Aristotle's psychology. He would no longer consider the characteristic mark of the psychical as residing in consciousness. He would accept * * * the conception which regards the soul as 'the first entelechy of the living body.' And the soul, so considered, would appear in all truth as 'that empirical concept of which

everyone makes use who really and successfully cultivates empirical psychology and not barren speculation'" (216).

This is the doctrine of the soul which the neo-scholastic propounds in place of the Cartesian separation between body and soul. Man as we know him is not a pure consciousness, but a compound of mind and body. Results deduced from the conception of consciousness as isolated from the body are not applicable to the actual man. Psychology, so understood, is an arbitrary abstraction. What is wanted is an anthropology, based upon the fundamental thesis of the *substantial unity of man* (295). The soul of man is the soul of the entire man, and is not to be found wholly in consciousness. Its primordial function is not thought or feeling, but the 'informing and animating of the matter of the body.'

Between the soul and its acts we must, on metaphysical grounds, assume the existence of *faculties*, substantially distinct from the soul (304). Since a faculty is simply a means of arriving at an act, there are as many separate faculties as there are types of the soul's action. There are five groups of faculties, those of organic life, those of sensory knowledge, those of intellectual knowledge, those of will, and those of locomotion. Feeling and will, on the other hand, are not separate acts, but parts of the same act. Feeling is but the passive side which appears in every state of consciousness, but which has been overlooked because the mind, in its Cartesian isolation, was, almost of necessity, conceived as the source of its own ideas and as altogether active.

In conclusion, the author sketches the history of the neo-Thomistic movement, recognizes that the dogmatic method of the old scholastic philosophers must now-a-days be replaced by a critical method, and urges his colleagues to foster experimental psychology, for which their own fundamental conceptions provide the only logical basis.

The above summary of the author's principal line of argument passes by several interesting chapters, such as those which defend the conceptions of supra-sensible knowledge and of finality immanent in nature. To criticise the author's views would lead us too far afield. His style is clear and attractive. His argument is skillfully conducted, and is well worth some attention from those who have been brought up on an idealistic diet. The main defects of the book lie in the direction of inadequacy. His statements of opposing views are admirably fair and objective, so far as they go. But he does not do justice to the positive side of the work of Kant and his successors. Still less is his summary statement of certain scholastic doctrines adequate to carry

conviction. For a fuller statement of them the reader is presumably referred to other books by the same author.

ROBERT S. WOODWORTH.

COLUMBIA UNIVERSITY.

La Personne Humaine. L'ABBÉ C. PIAT. Paris, Alcan. 1897. Pp. 401.

The theme of this work is suggested by the contradiction between the representations of human personality formulated by the traditional spiritualistic philosophy and those current in modern scientific psychology. According to the former the human person is a unique, indivisible, self-identical and permanent entity, actively producing and supporting its states and possessed of intrinsic capacities of reflection, in virtue of which it is rational, responsible and free. According to the latter the conscious self is a resultant of the play of a manifold of elements, the coördination of which constitutes its unity, and this coördination is never absolutely complete, but is capable, under pathological conditions, of such profound disturbances that two or even more personal consciousnesses may successively or simultaneously arise in connection with the same bodily organism; the organism itself, and not an independent conscious entity, is then commonly regarded as the substantial bearer of the mental life, which latter is represented as everywhere conforming to general principles of evolution and subject to the inexorable necessity of nature. We have here a clear, forcible and eloquent apology for the spiritualistic tradition face to face with the newly-discovered or newly-emphasized facts of science and in conflict with dominant scientific hypotheses.

The argument falls into three main divisions, entitled respectively Perception, Reflection and Responsibility. The first maintains by the usual appeal to the unity of consciousness, recognitive memory, etc., the original unity and persistent identity of the self, and criticises the evidence to the contrary in the facts of double consciousness. The second maintains the unique originality of the power of the human mind to think of and through universals, and criticises the evidence for the derivation of this faculty on evolutionary principles from an organic process, from instinct or from the language of the lower animals. The third maintains the reality of freedom relatively to the moral ideal as a living and concrete perception, and, explaining the consciousness of responsibility as dependent on a number of elements independently variable, sets forth the causes and consequences of its enfeeblement with impressive reference to certain features in the dark obverse of modern civilization.

From a scientific point of view the most valuable parts of the book are probably the critical. The criticism of the phenomena of double consciousness follows the lines made familiar to us by Professor Ladd in demanding more accurate observation and description of the facts and in explaining the accredited phenomena partly as pure automatisms and partly as changes due to distraction of attention in the field, rather than in the subject of consciousness. The criticism of the evolution theory in the second part is also unquestionably acute, though in insisting so strongly on the 'fait décisif' it seems to overlook the vast heuristic importance of a conception which may fall far short of the verification desired. Still, as over against a certain tendency to elevate a scientific theory into a scientific dogma, it is not bad to be reminded once in a while, even at the risk of some exaggeration, of its actual shortcomings.

As to the positive constructions of the book, the questions involved are so many and so complex that it is difficult to say anything without saying much, and any extended discussion would be here out of place. A few words on one point only must suffice. The contradiction which the author notices at the outset is certainly one which occasions no little perplexity to the student and the clearing up of which is a task worthy of a philosopher. But when, fresh from the reading of Ribot and Binet or, say, from the penetrating chapters of Mr. Bradley, one goes for more light on the unity and identity of the human person to the pages of this book, one can hardly help feeling, with all admiration for the lucidity of the style and the intellectual and moral tone of the discussion, that the real difficulties have not been met after all. One admits, of course, the unity of the consciousness, in each particular act of attention, what James calls 'the unity of the passing state,' whether the state be one of perception, of recognitive memory or what not; but to find in this the evidence of the unity and persistent identity of the concrete self, seems trivial. For this unity of consciousness, even if we include in it the invisible unity of the subject 'I,' surely is not the self, the person, of which we and the writers whom M. Piat opposes are thinking. And when, taking up the concrete self in all the complexity of its changing content and the variety of its aspects, we ask after its nature and significance, then it is that our question becomes burdened with all the weight of the problems of biological and psychological science and of social and metaphysical interpretation. It is noteworthy that, while claiming for the ego an existence as a permanent unity, on the direct testimony of consciousness, M. Piat refers the question of its substantiality to theology and faith. So far as this

points to a higher criterion of judgment than that furnished by empirical psychology, it embodies a true instinct. For the ultimate meaning of personality is found, not in the facts of consciousness, but in ideals of the will. As Hegel said, the great thing is not to be a person, but to become one. But, if this is so, then the first thing to do is to discriminate as far as may be with the utmost clearness the different questions at issue and the different points of view from which they may be legitimately regarded. It is more than likely that when the presuppositions and relativity of the different standpoints are fully understood, the disputes between spiritualists and phenomenologists, metaphysicians and scientific psychologists, will largely disappear. Philosophical dogmatism now as of old renders discussion interminable.

H. N. GARDINER.

SMITH COLLEGE.

Citizenship and Salvation, or Greek and Jew. A Study in the Philosophy of History. ALFRED H. LLOYD. Boston, Little, Brown & Co. 1897. Pp. 142.

Stevenson, in one of his essays, remarks that the purpose of a preface is to give the author of a book the opportunity, after his labors are over, to appear before the public with his plans, and proudly proclaim the nature of his achievement. The reviewer of *Citizenship and Salvation* would be much surer of his ground if Dr. Lloyd had availed himself of this privilege of the author and not sent this interesting, but very obscure, little book out into the world without a prefatory word. The work is divided into three parts, entitled, respectively, 'The Death of Socrates,' 'The Death of Christ,' and 'Resurrection.' It is, as its secondary title indicates, a 'Study in the Philosophy of History,' and it is conceived in a thoroughly Hegelian spirit, although entirely independent and original in plan and execution. It is also called by the author a 'biological study of self-denial,' and might equally well be styled a metaphysical study of self-realization. It is not a 'supernatural' or 'unnatural self-hood,' however, that is realized in self-denial, not 'a self that originally was not.' Self-denial is 'the way to the expression of an already active life, of an already living ideal.' In other words, the self that gets 'fulfilled' in the historical process through self-denial was from the first 'secure' and 'active.' History, progress, means the record of successive self-denials, rendered necessary by the incompleteness of ideals and the consequent clashing of opposing tendencies, which ever result in self-fulfilment. Socrates, for example, the real Socrates, was 'vitaly present in the life of Greece from the

very beginning; he was 'the inner motive of Greece that had in spite of all determined her destiny from the very beginning.' He was alive in Greece long before Phænarete gave him birth on the slopes of Lycabettus, and he continued to live after he had drunk the poison, getting fulfilled even in his own death and in the death of his race in the triumph of Rome. The contradiction which led to the tragedy of Socrates' death was the contradiction between the worldly life, the 'miserly' life, which takes means for end, and 'the life apart from the world, which assumes that the end will realize itself.' Either attitude alone would check fulfilment. Divorce of means and end meant their reunion in an historical movement, which Philip of Macedon and Alexander in fact inaugurated, and which Rome completed. Of this movement the philosopher was Aristotle, who taught that the soul is not an end by itself, but the end or purpose of the body, and that, similarly, the world is to be regarded as the incarnation of reason. When reason is regarded, however, as the world's, it comes to be regarded as no longer man's, and thus forgetfulness becomes 'the successor of reason in man,' and man himself is then considered as 'but a means to the world's end.' The universal empire of Rome, with its militarism and mechanism, is the inevitable outcome. Thus Rome completes the work of Alexander and Aristotle.

The way is now prepared for Christ, who as the 'World-Reason' (the 'Word Incarnate,' the 'revealed ideal') is 'the liberator of the world.' This Christ, however, is not merely the historical Jesus, but, again, as in the case of Socrates, a motive always present, although in him become at last, in a special sense, active. This idea of a World-Reason revealed unto men, is the self of Christ that triumphs in Roman Christianity. When we turn to Judea, moreover, we observe a conflict similar to that which existed at Athens; there we find the same 'co-operation of symmetrical opposites.' Jewish idolatry of the past had come to be pure formalism, lacking all vital content; and Christ, as the Messiah, expressed in life 'an as yet unrealized ideal.' The respective attitudes of Christ and his people were, however, but contradictory and one-sided aspects of the single activity which constituted the national motive of the Jewish people. Hence the collision was inevitable, and in that collision—the crucifixion—the Jewish ideal is set free. And here we come upon a very ingenious theory of our author's. The motive of the Jewish people found also its expression in money lending. Money, as a commodity, is the treasured past which the Jews idolized; lending, on the other hand, affirms the future as motive. 'In money-lending the confusion of future with

past found expression, and a national life, so long isolated, so long deprived of participation in distinctly worldly affairs, was set free, the people turning their necessity into opportunity' (p. 81). And so the Jews became Christians in their own—'a very worldly way.' They with their talent for money-lending, and the Christians, with their spiritual other world, severally conquer Rome.

Let us apply these ideas. Rome had become, before the appearance of Christianity, a military government, a mechanism. Each part, each citizen, of such a government says in effect: 'I am not, because we are all one and equal.' Yet each feels, when the mechanism moves, as the soldier feels after the battle: 'I did it' (p. 90, 91). 'A sentient mechanism is a whole which upon action breaks into a group of microcosmic reproductions of itself,' that is, it becomes an organism (p. 91). Christianity, therefore, was able to interpret Rome unto herself, for Christianity is summed up in the word organism, which Dr. Lloyd 'likes to call the Christ-motive.' Now there are two 'chief incidents of all activity,' sanction and interpretation. (This is introduced with an 'of course.') Socrates 'sanctioned' the Roman empire, Christ 'interpreted' it. But the interpretation is, as always, fatal. Organism and mechanism cannot co-exist. The remaining history of Rome is simply the record of a process, in which the leaven of the idea of organism is spreading more and more. Philosophy, with her dispute over universals, over substances and monads and *a priori* forms, tells the same story. And Kant is the last great Roman philosopher.

In the third part of the book, conclusions are drawn—not without an apology. Democracy is seen to be the goal, and at the same time the fulfilment of monarchy. It is also the consummation of the 'Christ-motive' organism. In a democracy, each citizen is, in fact, a *parvus in suo genere rex*, each has 'imperial rights over his own complete self-expression.' At the same time, each becomes a mediator for, represents, all the rest, in his own individual way. In order that he may properly do this, however, he should have 'credit' precisely in proportion to his power to apply the world's forces, and the bank should be merely an institution for gauging this credit by accurately measuring each man's individual 'capacity for action.' When banking reaches this perfect stage, every capitalist will be a laborer, and every laborer a capitalist. The church will undergo a similar resurrection. It will cease hoarding the future, as the bank will cease hoarding the past, and turn its attention to the 'more vital expression' of the soul, here and now. Prayer is then simply 'the earnest, hon-

est, trusting definition of the sphere of one's activity; it is 'science becoming motive, or mind liberating soul.' And, on the other hand, 'ritual' is simply 'body expressing soul,' the action which prayer sets free.

To criticise Dr. Lloyd's work in any complete way would require a book larger than the volume before us. I shall confine myself therefore to a few of the more obvious reflections that suggest themselves. It is probably safe to surmise that no one will be convinced by the book. It is far too brief to prove its position, and far too long for a mere statement of it. There are numerous repetitions—a defect which the author himself recognizes. Now, very frequently these repetitions concern just those matters about which we should like to have more light, but the repetition does not give the added light. Startling assertions are frequently made as matters of course, a fact which intimates that Dr. Lloyd has as yet let the world only into a little corner of his thinking-shop. One cannot but feel that there is much that is fanciful in our author's reasoning, as, for instance, in his discussion of money-lending as 'left-handed' Christianity, and in the whole treatment of banking. So in the characterization of historical events and personages, one is sure that the facts are distorted, or, at least, but very partially presented, in order to fit the formula. The description of Socrates, for example, as a 'spendthrift,' taking end for means, and standing for 'abstract spirituality,' is Socrates twisted so as to form the proper antithesis to his contemporaries, regarded, also by a *tour de force*, as 'misers taking means for end.' Again, when we are told: 'in the nature of organisms, as he who runs may read, are the primal teachings of Christ' (p. 92), we are hardly satisfied to forego the evidence. To try to show that history had to be as it has been, is a dangerous and difficult, if not impossible, undertaking. Dr. Lloyd would seem not to have a proper appreciation of the magnitude of his task. He slips over and around obstacles smoothly enough, but for this very reason leaves the impression that much of his writing is mere word-play. It is often hard to see what he is thinking behind his phrases. Words are not used with that consistency which logical procedure demands. We find ourselves reading of Christ, the motive of his people, and anon, without warning, we are dealing with the historic Jesus of Nazareth. There is a similar shuffling in the case of Socrates. The term organism is used with great vagueness. The general objection to this whole way of thinking the universe under the form of an organism—the objection, namely, that the individual is lost in the process, that his significance, his freedom, is destroyed—is

lightly passed over with the remark, repeated several times, with slight variations, that when we say that history 'required' the appearance of a given man, at a given time, we mean also that his own true selfhood required the same thing: (p. 61) that 'internal sanction,' corresponding to 'external stimulus,' frees us from determinism. But this 'true selfhood' appears, after all, to be a sort of spirit of humanity behind the scenes, the 'inner motive' of the life of the people, and, thus considered, the doctrine becomes 'as vague as all unsweet.' One finds, however, many passages in the book which dimly suggest that Dr. Lloyd has a message of which he has not yet succeeded in delivering himself. *Citizenship and Salvation* is a program, and it is to be hoped that Dr. Lloyd will live to carry it out. Only we cannot refrain from adding the further wish that in the meantime he read more French and less German, so that the message may be more intelligible when it comes. What, for instance, could be more hopelessly obscure than pages 72, 73?

CHARLES M. BAKEWELL.

BRYN MAWR.

A Study of a Child. LOUISE E. HOGAN. New York, Harper & Bros. 1898. Pp. x + 220.

This is so distinctively a popular book that one hesitates to offer a review of it for publication in a psychological journal. But a justification for so doing is to be found in Chapter I., wherein the author tells us that "the few facts that were noted (during the first year) may be of greater interest possibly to psychologists than to the general reader." So she presents her observations to psychology. To quote from these records will serve to illustrate the nature of them, and the psychologist will perceive that he is not to expect to find trustworthy observations, critically considered. "When the child received his first bath he lifted his head unaided from the lap in which he was lying, thus showing to the popular mind an early inclination to know what was going on about him * * * and to the psychologist great promise of brain powers" (p. 15). On page 16 is noted his objection to a Raff concerto for the violin and piano, and his toleration upon the same occasion of Handel's Largo. This observation, at least in its present form, is not available to psychology except as having the value of an impression; for it lacks the verification which it should have received from subsequent observations, or from an alternation of the concerto and the largo in order to determine whether the child's feelings changed with the change in the music. The main feature of the book is a series of

500 drawings by the child, executed by him during a period of some six years. These show an advance in the acquirement of manual dexterity, and an increasing appreciation of detail in the objects represented. The subjects chosen by the child for the exercise of his skill are also noteworthy, as indicating the direction of his interests. But we are not told (except in the case of the locomotive) whether drawings were often made for him, so that he followed or was helped by a copy; whether he drew from an object, from the memory of an object, or by all of these methods. Many of the drawings are obviously imaginative. These should have constituted a separate series. The want of system in the arrangement of the cuttings and drawings is to be regretted, and is a hindrance to their usefulness.

The language record also is fairly full; yet here only a time record is given, from which one learns that the child was able to say certain things by a certain date. It is a pity not to have formulated the records for correlation with other observations on child language.

The book is full of suggestions as to methods of inculcating desirable habits and various virtues, which will attract both kindergartners and parents, and it leaves on one's mind the pleasant impression of a happy, lovable child.


KATHLEEN CARTER MOORE.

PHILADELPHIA.

Die praktische Anwendung der Sprachphysiologie beim ersten Leseunterricht. H. GUTZMANN. Berlin, Reuther u. Reichard. 1898.

Every medical man is more or less interested in the physiology of speech in proportion as he is called upon to study the many defects which are met with among school-children, and the conviction is forced upon him that by good or bad methods of instruction latent tendencies to such defects may be either developed or eradicated. From such a practical acquaintance with the results of imperfect training arose Dr. Gutzmann's interest in the theoretical problems discussed in this work.

The monograph is divided into four parts: first, an historical review of the opinions of educational writers concerning the place of the physiology of speech in school instruction; second, the psychological justification for the study of speech physiology, and its practicability as a school method; third, the hygienic value of a physiological method in teaching to read; and fourth, the practical application of physiological principles in school instruction.

The earlier treatment of the problem was based on fanciful analogies between the form of the printed letter and the adjustment of the organs necessary in pronouncing it, as when Bonet the Spaniard remarks that the shape of the letter B, with its two semicircles meeting the perpendicular line, signifies the closing of the lips involved in uttering it, and that the letter A is formed like a trumpet  to indicate that the letter must be pronounced with open mouth and constricted throat, which latter, however, as the cross-bar indicates, must not be wholly closed. Passing by these, we find that a long series of those who grasped the real significance of the speech-learning process have urged, or adopted, the physiological method of instruction. Graser, Fechner, Böhme, Krug, Grassmann—all these made earnest efforts to apply the principles of speech physiology in their instruction. Krug, the most explicit and insistent, demands that every child shall be made to construct each vocal element with a clear consciousness of the various adjustments of the organs involved; this exercise shall precede the actual instruction in reading. Krug's principles are intricate, his process, at least at first sight, artificial, and his method tiresome and wasteful. It is necessary to seek more simplified and practicable ways of applying these principles in school instruction.

The psychological justification of this method lies in the nature of the processes involved in learning to speak or to read. The combined process involves the activity of five brain-centers with their respective tracts: (1) The perceptive centre and auditory nerve-tract; (2) the motor centre and nervous tract connecting it with the mechanism of speech; (3) the visual perception-centre of the movements of speech and writing; (4) the kinæsthetic perception-centre which makes aware of the adjustments of the organs involved in speaking and writing, and (5) the motor centre by which the movements of the hand in writing are produced and directed.

The child learns by imitation; in speech this is chiefly through the ear, but not solely; the eye also participates. The child imitates movements of the lips when soundless; blind children come to speech later than the normal; of those who have lost their hearing some rapidly recover power to communicate by reading the lips. In German deaf-mute schools the sole method of teaching vocal language has been by the use of kinæsthetic and visual sensations in acquiring direct conscious control of the mechanism of speech. In blind deaf-mutes the sense of touch replaces vision in connection with kinæsthesia.

The objection may be urged that such a method is not practicable;

it is too confusing and burdensome, and can be applied only individually, not to classes. In reply, the success of deaf-mute instruction, and the relatively rapid progress of the pupils, demonstrate its possibility. By this method idiots have been taught to speak when all other means had failed. (v. Piper, Gutzmann, etc.) In normal fully-endowed children the progress should be correspondingly more rapid. As a matter of fact, it has been successfully used with young children and with stutterers to the number of nearly 400 by the elder Gutzmann. The objection is valid only against particular forms of the method which have been employed in the past, a disability which a perfectly possible simplification will remove.

The hygienic value of the physiological method of instruction in reading will be found in the correction of a long list of defects in speech and the prevention of a still greater number. There are 80,000 stammering school children in Germany. The proportion increases greatly with the age of the pupils, the number in the highest grades in some schools reaching three times that in the lowest. The statistics from half a dozen cities show that the most rapid increase takes place between the ages of seven and eight years, that is, immediately after the first instruction in reading. Of these a large part on their entrance upon school life were not developed stutterers, but showed only a tendency to such defect. It lies in the power of the teacher to correct this predisposition by training; else the habit, which is highly infectious, will be fixed through imitation. The still imperfect control of speech when the child begins school life, combined with frequent tendency to stuttering and lack of self-confidence, affords at once the condition for the establishment of all sorts of defects. The root of all such troubles lies in the imperfect control of the mechanism of speech, which has all along been practically met by directing attention to the processes involved and endeavoring consciously to perform the correct movements. Diesterweg and Gutzmann especially have urged the use of this means as a corrective, advising systematic practice in breathing, vocalization and articulation.

The practical application of these principles should not precede the teaching of reading, but should accompany and illustrate it from the first moment. The question of method presents three problems: (1) the means which the psychology of speech reveals for awakening the right physical images; (2) the way in which the individual organs can best be exercised; (3) the arrangement of the course of instruction in response to this demand.

The means are hearing, feeling, seeing. Clear apprehension of the

sound to be produced is necessary, since by it the correctness or incorrectness of the adjustment is chiefly to be judged. The child should be trained to observe, by direct feeling, how the mouth and throat are adjusted in speaking, for by means of these kinæsthetic images the movement is afterward produced. He should also know the form taken by the vocal organs in pronouncing the elements of speech, for by this means he is directly assisted in the production of the specific sounds desired.

The author does not propose the substitution of a radically new method in teaching, but only the introduction of a rational system of training in vocalization and articulation, in connection with the use of illustrated primers and photographs of the positions of the vocal organs in articulation. A plate of twelve such pictures accompanies the monograph.

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HARVARD UNIVERSITY.

Die Ideenassoziation des Kindes. PROFESSOR ZIEHEN. Berlin, Reuther u. Reichard. 1898.

In his introduction Professor Ziehen reviews the experimental work done concerning the association of ideas in children, and gives full bibliographical references to researches upon the nature of association in general. The work reported on was confined to children from eight to fourteen years of age, and considered four things: the determination of the child's store of ideas; of the nature of associations resulting from a given initial idea; of the rapidity of the association process, and of the influence of special conditions, such as fatigue, upon the rapidity of association. The first inquiry was a preliminary one. Of the three principal questions of the investigation only that concerning the association process is taken up in the present monograph. The initial idea was suggested by an object seen, heard, or felt, or by a word. The words were arbitrarily chosen monosyllables, usually concrete ideas, with occasional terms of relation (*e. g.* 'similarity') of processes ('storm') and proper names.

Concerning the form of association the question of chief interest is whether the process of association in the child shows a greater or less tendency than in the adult to special fixed association groups.

Our earliest associations are, without exception, spatially and temporally determined individual ideas. From these are derived spatially and temporally indeterminate object ideas. In adults the word (except in the case of proper names) awakens throughout universal

ideas; to give individual worth it is necessary to add a definite article or pronoun. The author's distinction here is a logical one, but it is doubtful if such a discrimination between adult and child can be drawn psychologically. The image, or psychological representative, must be without exception a concrete image, definite or indefinite, derived from individual past experiences. Accompanying this psychological element, however, is the awareness that the image *means* a type and not an individual. But such knowledge of his meaning the child of eight already possesses. The truer distinction—and perhaps that intended—is that in the adult the concrete image has less localization; it hangs before the mind as an isolated thing, but not, therefore, as a universal.

Verbal associations were rare; only in one case were they frequent, where they formed twenty-four per cent. of the total. Among adults such forms are much richer and more frequent. Of verbal associations the most usual form was word-completing, *e. g.*, *Bett-federn*; *Post-Karte*, etc. Rhyme associations were rare, *Schlange*; *Zange*; *Fisch*; *Tisch*, etc., but individual cases were found with almost every child. One noticeable type smacks strongly of the school and its training, *e. g.*, *Bett* is written with 'tt'; *Macht* is written with a capital.

In the case of adults not only is the representative image of a concrete term a general one, but almost always the image which it calls up is likewise general. It is astonishingly different with the child. The author says: "I was prepared for a relative predominance of the individual association form. Of the degree of this prevalence I had not the slightest suspicion * * * * Most of the children connected with almost every stimulus word an individual idea, and with this again an individual idea, and in many cases both were spatially determined." The percentage of individual associations decreases with the age of the child; in the third class it is seventy-two per cent.; in the first, sixty-two per cent.; among adults the author found it to be on the average ten per cent., in regard to analytic and synthetic associations the elementary idea never aroused another elementary hetero-sensorial idea (*e. g.*, *sweet white*). This is natural; it awakens always the totalized object association (*e. g.*, *sweet-sugar*); this point suggests such phenomena as colored hearing, of which the author makes no mention, and the question whether they are of later development and not present in the imagery of children of eight to fourteen years of age. The elementary idea arouses a composite image four times as often as a simple; the type is *red-blood*, not *red-green*. This also is natural; the child

finds red combined in the concrete with elements of the other senses constantly, but seldom with green. The composite idea arouses most frequently (59 % of all cases) as its associate another composite which bears no relation of partiality to the first (*e. g.*, *window-door*); next to this, but far behind, comes its association with a greater composite of which it is a part (*e. g.*, *window-wall*); very seldom is this latter relation reversed (*e. g.*, *window-window-sash*).

The visual type predominates much more than among adults. Affective partitive ideas are very rare (*e. g.*, *gut—nicht gut; thut Weh*; etc.). In spite of the variety of content the form of association is always *contiguity* in the wider sense. No case of pure (indisputable) association through resemblance was observed. Associates far fetched in space and time are found much oftener than with adults. With adults familiarity is predominant; with the child congruity plays a much greater rôle; on the other hand, distinctness and constellation bear a much less important part. In closing the author recalls again the difficulty in tracing all the linkages of association upon which the very form depends, since the child himself forgets, and the consequent need for extended and patient investigation for the determination of these problems.

ROBERT MACDOUGALL.

HARVARD UNIVERSITY.

Psychologie collective et Psychologie individuelle. RENÉ WORMS.

Lecture delivered before the Paris Academy of Moral and Political Sciences, November 12 and 19, 1898. Pp. 35.

These are by no means the first helpful words which psychologists have received from this eminent French sociologist, but they contain the most pointed bit of advice which he has addressed to them directly.

Starting with a reference to the rapid growth of sociology in recent years, he passes with a word the evident dependence of sociology upon psychology, and proposes to trace the influence of sociology upon psychology, in the formation of a collective psychology and in modifying the psychology of the individual, and then to sketch the outline of a new psychology which would recognize these changes. He is not here concerned with the metaphysical reality of the collective mind, but with the scientific inquiry into the causes of such common phenomena as the mind of a nation, a family or a crowd. He finds two sets of influences always present, a common environment and the reciprocal influence of the members of a group upon one another. The soil, climate, and productions of a country, for instance, arouse numer-

ous common sentiments in the minds of a people, while the social relations of family, friendship, religion, politics and education have an equally large share in the formation of the national mind. In the family the same influences are at work, together with a new element still stronger, that of heredity. Men engaged in the same industry come to have mental characteristics which are distinguished from those of the national type in the same twofold way, by the more limited and therefore more intense effect of the common environment—city, village, shop and fellow workmen, and by the particular stamp of that industry. The various groups thus mentally differentiated within the nation may be divided into four classes: those founded on blood-relationship—families and races; second, those of locality—cities, villages and provinces; third, those of industry—farmers, mechanics, merchants, etc.; fourth, those based on intellectual interests—all sorts of political, religious, literary and social organizations. A reference to various French writers on the state, family, city, provincial life and the life of workingmen shows that a collective psychology is already an established fact. Few of these writers are professional psychologists, and much remains to be done, but the way has been mapped out. Development will follow naturally.

In showing how sociology has influenced the study of individual psychology attention is confined to the higher mental faculties of reason and free will. Reason, as the faculty of general ideas, is of practical value in the social life, since every individual is making constant appeal to laws and general ideas, but it is also social in its origin. Man gets his first general ideas from the constant recurrence of phenomena which pass before his eyes, but he is much more influenced by the character and action of the men around him than by the cosmical or biological elements of his environment. Therefore, the first general ideas are social. The preservation and development of these ideas is in turn possible only through the medium of language, another social function. In short, human reason is penetrated with social elements.

Free will is impossible apart from reason. Therefore, free will, in its nature, shares the social elements of reason, while the field of its activity is the social world. Its highest aim is the moral elevation of humanity; its standards of right action are social standards; its rules of life those which can be adopted by all right wills. Thus the factors in the creation and development of human personality are almost wholly social. Heredity may furnish the first elements of our being, but education is equally important in the formation of our character

and minds, and in adult life it is in social relations with our fellows that our personality is developed—by imitation, opposition and adaptation. The larger part of individual mentality is a product of our collective existence.

But although the social mind has become an object of scientific study, and the individual mind is shown to be largely a product of the social life, there are objections to setting up a collective psychology as opposed to individual psychology. Collective psychology is, in the last analysis, psychology of individuals, while the mental life of any individual can be understood only in the light of the social life. All psychology is at the same time collective and individual. Secondly, our ordinary psychology, which is generally called individual, is anything but individual. It deals with general principles which are true for all men, and is even more comprehensive than collective psychology.

A better division of psychology would study separately the three sets of elements which we have found entering into each personality: those common to the whole race; those common to the group of which he is a member, and those which make up his individual personality. First, we would have a general psychology of the mental faculties common to all human beings; second, a special psychology, or what has been called collective psychology. The word 'special' brings out more clearly the essential nature of these researches, that of distinguishing between different groups of men which from the social point of view constitute different species; third, an individual psychology which would study the particular mental life of concrete individuals, the normal development and crises of intellect and heart, their natural tendencies, their influence on associates, and the net result of their mental existence.

Should this division be adopted, the younger science of sociology would render a distinct service to psychology, but at the same time would be doing itself a good turn. The results of psychology thus specialized would be far more valuable than the universal and abstract principles of the present psychology. The second of the proposed groups would be of especial value to the sociologist. In the mind of the nation he would find the general causes of its economical, moral, and political organization. In the mental characteristics of its families, cities, industries, and social groups he would often find the explanation of the details of this organization, and also of the strifes and internal difficulties which the national life has to surmount. At the same time the first group would give him the general mental characteristics of mankind, and thus help explain the striking similarities in the de-

velopment of different nations, while the third group would throw light on the question how the intellect or will of a single man sometimes transforms an industry, a whole region, or perhaps a nation.

Our first impression is that, so far as the making of books is concerned, the second of these fields belongs to the sociologist, while the third is the peculiar province of the man of letters. Our second thought is that the radical reforms suggested in psychology have already taken place. Present psychology is not confined to abstract or general principles. It has had no difficulty in absorbing sociological doctrines, if it has not succeeded in absorbing the sociologists themselves, and it has also picked up a few facts from physiology, biology, anthropology, history, and other sciences. Nine-tenths of all the large and increasing literature of psychology belongs to the second or third of these groups—practically all of the so-called new psychology with its experimental work, child study, educational investigations, animal psychology, and abnormal psychology, with its suggestive researches in hypnotism, insanity, and the subconscious realm. A good beginning has also been made, especially by French psychologists, in the study of individuals; for instance, noted writers and men of skill. In fact, it is impossible to carry on psychological investigation and add to the sum of the truths contained in the first group except through the study of the concrete individual. In this particular psychology is not different from geology, physics, chemistry, or any of the other sciences.

But it is feared that there are many teachers of psychology in America, as well as in France, who imagine that when it comes to the classroom psychology is radically different from the other sciences, and that here general truths may be made interesting and profitable quite apart from the concrete facts. If a fuller recognition of this threefold division shall avail to inspire teachers with the newer spirit of their science, and bring the student into closer contact with concrete mental facts, making them all sociologists and giving them all the literary insight into human nature, and awakening them to the practical possibilities of psychology in the professions and in the daily life of every individual, psychologists will, indeed, be grateful to the sociologist.

But it is by no means clear that anything would be gained by attempting to introduce this threefold division into the text-books used in the ordinary courses of psychology. Where only one course is given, at any rate, it is better to do justice to the three elements simultaneously. There is more crying need for improvement in methods of teaching than for wholesale changes in text-books.

CHARLES B. BLISS.

O wahaniach w natezeniu minimalnych optycznych i akustycznych wrazen (zur Erklärung der Intensitätsschwankungen eben merklicher optischer und akustischer Eindrücke). W. HEINRICH. Reprint from the Bulletin of the Academy of Sciences in Krakau. November, 1898. Pp. 18.

This is an abstract of a paper which reviewed the whole discussion of the fluctuation of minimal visual and auditory sensations and presented the results of the author's investigations, some of which at least were previously described in the *Zeitschrift für Psych. und Phys. der Sinnesorgane*, Vol. IX. and XI. According to Heinrich the fluctuations with visual stimuli are definitely proved to be due to the constant fluctuations taking place in the curvature of the lens, while there is every reason to believe that the fluctuations of auditory sensations are caused by the effect of breathing upon the tension of the ear drum. Experiments with a carefully trained observer who had lost both ear drums failed to reveal any fluctuation. Microscopical examination showed that the ear drum does move outward with every inspiration and inward with every expiration. A graphic record of the breathing and the auditory fluctuations showed that with deep breathing the number of fluctuations corresponds with the rate of breathing. With normal breathing, out of fifteen respiration periods ten corresponded to fluctuations in sensation. The author thinks that a more exact knowledge of the movements of the ear drum will furnish a complete explanation of the phenomenon.

One of the most striking announcements is that no fluctuation could be detected when the faint sound was a steady tone instead of a watch tick. These experiments were made upon only one person, and it is hard to believe that this will be found true for all persons under all conditions.

CHARLES B. BLISS.

LEONARD'S BRIDGE, CONN.

Society's Need of Effective Ethical Instruction in Church and School, and the Suggestion of an Available Method. E. M. FAIRCHILD. *The American Journal of Sociology*, January, 1899. Pp. 433-447.

The writer describes his method for the visual instruction of ethics in the public schools. By means of the camera and lantern slides scenes illustrating the various practical ethical problems of child life, quarrels and fights, work and play, the sneak, the thief, the bully, the cry-baby, the general good-for-nothing, are shown to the children.

while the teacher, in a carefully prepared lecture, describes the proper adult feeling called forth by the successive pictures.

CHARLES B. BLISS.

The Dawn of Reason or Mental Traits in the Lower Animals.

JAMES WEIR, JR. New York, The Macmillan Company. Pp. 234.

The author begins with the following definition, "Mind is a resultant of nerve, in the beginning of life, neuro-plasmic action, through which and by which animal life in all its phases is consciously and unconsciously, directly and indirectly, maintained, sustained, governed and directed." He holds that conscious mind originated in sensual perception thousands of years after unconscious mind. The book treats of the following topics: Sense in the Lower Animals, Conscious Determination, Memory, Emotions, Æstheticism, Parental Affection, Reason, Auxiliary Senses, and Letisimulation, and the whole is followed by general conclusions, a bibliography and an index. No writings recognizing the Weismann theories seem to have been consulted, for few, if any, appear in his bibliography, and he writes as if the inheritance of acquired characteristics had never been questioned.

The author has spent many years in observing and experimenting with animals, and reports some very interesting and valuable facts, but his long association with animals and habits of reading his own ideas and feelings into their actions make him partisan and uncritical. He has discovered that when dogs appear to be baying the moon they are listening to the echo of their own barking, and says that the 'dog's voice is exceedingly pleasing to himself,' and that this indicates a 'high degree of æsthetic feeling in the dog,' when the more natural explanation would be that he supposes that he is answering another dog. He thinks that animals can count, and even holds that a blind dog who recognized the loss when one of her six puppies was taken away soon after birth must have had an abstract idea of the number six. The most surprising example of uncritical judgment, especially for a medical man, occurs when he says of the mason wasps that "they possess a mental faculty which far transcends any like act of human intelligence; they are able to tell which of the eggs will produce males and which females. Not only are they able to do this; but seemingly fully aware of the fact that it takes a longer time for the female larvæ to perfect than it does the male larvæ, they provide for this emergency by depositing in the cells containing female eggs a

larger amount of food." It seems never to have occurred to him that nutriment may determine sex. He cites the recognition of one ant by others of its nest as an example of *memory*, although Lubbock has proved that this takes place when the ant has not been hatched in the same nest, but has been hatched elsewhere. In general, he thinks of but one explanation of a fact, or else accepts the one most favorable to the intelligence of his animal friends; hence his *conclusions* in regard to the higher mental activities of animals will have little weight in the present critical study of animal intelligence, though some of his *facts* are valuable.

His most important contribution would seem to be the work he has done in studying the sense organs and discriminative power of the lower animals. If he can prove to the satisfaction of other psychologists what he claims to have demonstrated he will be entitled to a high place as an investigator in this field. In most cases he gives few details in regard to his experiments, so that it is impossible to judge as to the care with which they were performed.

He holds that all animals can tell the difference between light and dark, even without an eye or optic nerve, as is shown by such instances as the blind fish from Mammoth Cave always seeking the darkest place in the aquaria. He holds that such low animals as jellyfish will follow a light, and that their so-called 'marginal bodies' are eyes, instead of ears, as others have claimed. He claims to have discovered rudimentary eyes in the star fish, oysters and worms, and holds that a snail has a cornea, a lens and retina, and can detect a white moving object like a ball of cotton, with which he experimented, at a distance of two feet and a black one at from twelve to fourteen inches, and that a crayfish can descry a man at the distance of twenty or twenty-five feet.

As to hearing he says: "It is highly probable that the majority of lower animals, especially those which are sound producers, can hear just as we hear," and that others can hear "by *feeling* the sound waves." He claims to have demonstrated the organs of hearing in a number of insects, and that only in the Lepidoptera and certain Hemiptera are they in the antennæ, as has been claimed by many entomologists.

He claims that animals have at least two auxiliary senses, "tinctumutation, the color-changing sense, and the sense of direction, or, as it is erroneously termed, the 'homing instinct.' Neither of these faculties is instinctive, but they are, on the contrary, true senses, just as hearing or taste or smell," and he thinks he has demonstrated the gan-

gion centers concerned in these senses. If Dr. Weir desires his claims in regard to the senses to be accepted by scientists he should publish further details, for naturally no careful scientist will accept such important conclusions till details have been given and the results verified by the experiments and observations of others. The present book is popular rather than scientific, as, indeed, the author intended it should be.

E. A. KIRKPATRICK.

FITCHBURG, MASS.

Vergleichende Untersuchungen der Sehschärfe des hell- und des dunkeladaptirten Auges. S. BLOOM, und S. GARTEN. Pflüger's Archiv, LXXII., 372-408. 1898.

This paper contains errors of carelessness in the parts that are open to the eye of the reader, and hence it fails to inspire confidence that the thousand and one little details that require constant attention in the carrying out of any investigation have not been neglected. The reviewer does not, of course, usually take the time to look for such errors, but in this case he is much struck to find, from the diagram on p. 404, that the visual acuity of an observer is, under certain circumstances, $\frac{4}{100}$ at a distance of 9° from the fovea, and has risen to $\frac{8}{100}$ at a distance of 8° from the fovea, and also that upon another occasion, the change from $\frac{4}{100}$ to $\frac{8}{100}$ of visual acuity takes place between the distances 12° and 10° from the fovea. Upon referring to the table which the diagram illustrates, it appears that this is purely an error in the drawing, evidently caused by substituting at one point millimeters instead of the degrees into which they are being converted. Any one is liable to make a momentary mistake now and then, but it is difficult to understand how so palpable an absurdity in a drawing can have withstood the inspection of the two authors of the paper (and also, no doubt, of the head of the Physiological Institute of the University of Leipzig). One is not surprised after this to find that there are errors in the making up of simple averages. And in glancing at the other pages of diagrams, one notices that, on p. 389, when the visual acuity should be twice as good for the bright adapted eye as for the other, it is represented as being three times as good; this causes such a discrepancy in the course of the two curves as occurs nowhere else, and hence it is here also very singular that the authors did not look back at their tables to see if it was justified. On p. 398 we are asked to believe that an observer is able to distinguish two dots brought gradually in from the periphery at exactly the same distance whether the dots are five or

eight millimeters apart—that is, that his visual acuity at $21^{\circ} 20'$ from the fovea is, in a certain measure, *both* 100 and 63, and that nevertheless a superiority of a degree or two in the distance at which definition takes place on the part of the darkness-adapted eye (that is, transferring to the above measure a superiority of 12) is sufficient to found theories upon! Again, we find from the table on page 388 that at 6° from the fovea and again at $13^{\circ} 30'$ the visual acuity of the darkness-eye remains exactly the same up to the one-thousandth of the unit—here the ordinary unit—while the objective illumination is increased to eleven times, to fifty-seven times and to seventy-nine times that which was employed at first. (At 3° and at 12° , on the other hand, there is shown a gradual improvement.) This, indeed, would be an addition to our knowledge of very remarkable interest if it were a result to be depended upon.

A paper so riddled with evidences of utter ignorance on the part of its authors of the precautions to be taken in observations on the powers of the human eye in the periphery (and of the control to be exercised by common sense) has probably never before found its way into print. But in spite of these grounds for suspicion one finds oneself capable of a fresh feeling of surprise on seeing that the summing up of the results of the investigation is quite in disaccord with the body of the paper. We are told in the summary that “as appears from all our experiments on central and peripheral visual acuity, * * * in spite of the objective illumination being extremely faint, and alike for both eyes (the bright and the dark-adapted), the dark eye, though it sees things much brighter, sees them much less sharply.” But as regards *central* vision, this was not the case at all for a very faint illumination, as the tables show, and as the authors themselves plainly state a page farther on. Thus it appears from the table on p. 388 that it was only when the lowest illumination tried had been increased 1,170 times that the bright-adapted eye saw better than the dark-adapted eye in the center; and the authors say on another page of this same summing up: “only a much more considerable increase of the illumination brought about for the center of the retina as well a superiority in the capacity of the bright-adapted eye.”

If we overlook these numerous marks of inadvertence on the part of the authors and treat their results, for the moment, as deserving of acceptance, they would appear to have made out that, starting with an illumination just invisible to the dark-adapted eye at the center: (1) the dark-adapted eye remains the superior up to eighty times that illumination as far as three degrees from the fovea, but either farther

out, or for brighter lights, the bright-adapted eye renders the better service; (2) this superiority of the bright-adapted eye becomes so small at a distance of 40° as to fall within the probable error of the observations, that is to say, to disappear entirely (a fact which is not drawn attention to by the authors); (3) nothing is gained for the dark-adapted eye by reducing the illumination for it until objects look no brighter than for the other eye. (It does not appear why a long investigation of this point was thought necessary, in view of the fact that it had already been shown that a diminution of intensity had no favorable effect upon vision for an eye in this state. A condition X having been shown to be superior to a condition Y, it would not seem, as a matter of logic, to be necessary to go through an investigation to show that it is also better than Y_1 , when it is known that Y_1 is never better and is nearly always worse than Y.)

The authors give no discussion of the theoretical bearing of their results, except to point out that, since there is no illumination at which the dark-adapted eye sees as well as the bright-adapted eye sees at its *optimum* illumination, the state of dark adaptation cannot be simply a state of non-fatigue. If our knowledge of the retina were still in the condition which it was in before we had gained any information about the growth of the visual purple or the descent of the pigment granules, this would be an interesting contribution. As it is, the result is simply what we had every reason to expect. The withdrawal of the pigment granules has for its evident effect the reinforcement of the faint light which enters the retina by reflection and refraction from one visual element to another; it would be very strange if the space-giving elements of the retina, whichever they may be, should not perform much better service when they are isolated in their beds of black than when they are subject to an influx of light on every side. Many of the recent writers on these subjects speak as if the night-adaptation of the retina were an affair of the visual purple only, and apparently forget the important change which takes place (and which cannot be without effect) in the position of the black pigment of the epithelium, a change which is entirely adapted to explaining the diminished visual acuity for a given subjective brightness of the night-adapted eye. This phenomenon does not apparently throw any light on the burning question whether the rods are or are not chiefly instrumental in the renewed vision that comes to us by night. It is only when the cones are known to be *hors de combat* by means of the night-blindness of the fovea, that we can be sure that we are dealing with rods only; the experiment made at this illumination goes

to show that what the rods gain in sense of brightness by the increase of the visual purple they more than lose in definition (*i. e.*, in space sense) by the loss of their separating pigment granules, but that (even when the increased sense for brightness is wholly counteracted by causing the night-adapted eye to look through gray glass) this superiority practically ceases at a distance of 40° from the fovea. It would be interesting to know if there is here also a diminution in the extent of retreat of the pigment granules.

There is every reason to expect that the cones as well as the rods should show subjectively some effect of night adaptation, for their change of size is a very marked phenomenon. This may easily account for the fact that there is some slight adaptation, if not at the center, still within the rodless region. We know now that the feeling of pressure is dependent upon a deformation of the skin and probably a change of concentration of fluids in which nerve-ends are immersed. Such a change of conditions might also easily follow upon the shrinking of the visual elements of the retina.

C. LADD FRANKLIN.

BALTIMORE.

A Study of the Sense Epithets of Shelley and Keats. MARY GRACE CALDWELL. Wellesley College Psychological Studies. Poet-Lore, Vol. X., No. 4, 1898. Pp. 573-579.

This study gives a careful tabulation of all the sense epithets used by the two writers. The first table compares the frequency of adjectives of the different senses—sight, sound, touch, taste and smell. The second shows the number of adjectives used figuratively compared with the number used literally. The third gives the number of adjectives of color, lustre and form, while a fourth compares the frequency of the nine colors most used. Sight stands first in frequency, sound second, while touch, taste and smell are less adapted to poetic use. Shelley writes less of the external and uses fewer sense epithets than Keats. A larger proportion of those that he does use are figurative.

CHARLES B. BLISS.

Dendro-Psychoses. J. O. QUANTZ. American Journal of Psychology, Vol. IX., No. 4, pp. 443-306.

Even a psychologist has to think, for a moment, what Tree-States-of-Mind may be. Dr. Quantz has in this article given in a valuable collation of facts an interesting view of the vegetable kingdom as it has affected the body and mind of man in all their manifestations—

emotions, customs, religion, medicine and poetry. In the first section he sums up the biological and anatomical evidence for the descent of man from some race of tree-climbers, and in the next—'Psychic Reverberations'—he outlines certain psychoses which, existing to-day, can be, he thinks, accounted for only by the supposition that we spent our lives in trees in some previous pre-simian existence. Such states of mind are, among others, fears of serpents, winds, thunder-storms, and the fear of falling. 'Tree Worship,' 'The Life Tree,' 'The World Tree,' 'The Paradise Tree,' are followed by the tree 'in Medicine,' 'in Child Life' and 'in Poetry,' the last being the least successful, as for adequate treatment it would require a volume by itself. The prominence of the tree in all these relations seems, however, hardly to be proved, even by the very wide range of folk-lore covered by the author; and the article at times falls very near being a mere catalogue of the uses of the word 'tree' and its synonyms, wherever they occur. It seems credible that vegetable life, being next in importance to animal life, should receive a secondary amount of human attention; but it seems, likewise, somewhat in the air to use these facts as an argument for the spiritual descent of the human soul from the sensations and reactions of tree-climbers. 'T were to consider too curiously to consider so. It is hard to tell upon what subject such an article might *not* be written, where analogy runs rampant and the result is an intoxicating series of similitudes, which, if regarded with any degree of credulousness, dazzle one with their bizarre aspect. From the fact that when two branches of a tree grow together again, or the twig of a bramble enters the ground again making a hole, they have a remedial power, why should we not better infer a belief in holes or circles than in wood? These examples seem just as likely to be instances of the importance of the circle in magic or of suggestion as a therapeutic agent. As raw material for poetry Dr. Quantz's article is most interesting.

WILFRID LAY.

COLUMBIA UNIVERSITY.

Hydro-Psychoses. FREDERICK E. BOLTON. *Am. Jour. Psy.*, January, 1899. Vol. X., No. 2, pp. 169-227.

Minor Studies from the Psychological Laboratory of Clark University. XII., XIII. and XIV. *Ibid.*, pp. 280-295.

In the first-named paper the author investigates the influence that water has exerted in shaping and moulding man's psychic organism. Evidences of man's pelagic ancestry are found in the fact that his embryo goes through all the stages of evolution. Thus he is, at one

time before his birth, practically indistinguishable from a fish. The brain and nervous system, the organs of circulation and respiration show structural rudimentary organs; and vestigial structures in man are cited to show the subaqueous existence of our infinitely distant ancestors. Of course, a great argument is the fact that there *are* amphibious animals, and that, when young, they are all aquatic; and another is in the 'animal retrogressions to aquatic life,' seen in the whale, seal, beaver, walrus and sea lion. 'Psychic reverberations' are felt by us even to-day in the hypnagogic phenomena of swimming, floating and jumping, and in the preference for suicide by drowning. In 'the primitive conceptions of life' water is seen to be important, and the theories of the Ionic philosophers are dragged in, in the section 'Water in Philosophical Speculation,' 'Sacred Waters,' with their oracular powers and superstitions; and 'Water Deities' are cited in great numbers, as are 'Rivers of Death' and 'Paradise' as a land beyond the sea. Water itself is animate in the superstitions of childhood and primitive culture. 'Lustrations and Ceremonial Purifications' by water, including 'Infant Baptism,' show the natural reverence one has for one's forebears. Even 'Water in Literature,' poetical and religious, is touched upon and found to be a great source of all kinds of emotion. 'The Feelings of People at Present toward Water' have been investigated by Mr. Bolton with a Clark University questionnaire which contained rubrics on 'Running Water,' 'Large Expanses,' 'Waves, Billows, etc.,' 'Children's Animistic Conceptions of Water' and the 'Earliest Feelings toward Water;' and the answers to these numerous questions are given to the extent of almost nine pages of fine print. The 'Pedagogic Significance' of all this is that children like to play in the water, and they ought to be allowed to do it; and that the human soul is benefited by communion with water. Here is the final sentence (p. 227): "The childhood of the race was spent in delightful contact with nature; the child, ontogenetically recapitulating the phylogenetic development of the race, craves instinctively for communion with nature."

The serious objection to papers of this kind is that the writer has not given proof of the applicability of his facts to his theory and to his theory alone. This would have been most desirable in an article consisting largely, if not solely, of a conglomeration of facts and legends, a mixture of science and folk-lore.

This remarkable paper and its fellow *Dendro-Psychoses* noticed immediately above, are beautiful examples of the way to make a syllabus seem interesting; but the possibilities of the continuance, *ad*

infin., of such lines of thought should be appalling to the mathematically scientific mind. For why should we not have articles on Aer-Psychoses, Geo-Psychoses and Omichlo-Psychoses, as air, earth and fog (particularly the last) must have exercised a great, in proportion as it is distant in time of evolution, and, therefore, fundamental, influence upon our thought. Supporting the thesis in Geo-Psychoses which we expect to see emanating from Clark (I give a few hints for the forthcoming article) would be 'Dust thou art,' etc., and the fact that so many people prefer to be buried in the earth rather than burned up; and the fact that a great many persons will have themselves cremated and turned into their constituent gases is a good point for the article Aer-Psychoses. The present writer confesses to an extraordinary fondness for similitudes and analogies, and that he much enjoyed reading Mr. Bolton's article, for it awakened in him many desires to drop books and seek some well-known swimming hole; but it is a pertinent fact that, while reading *Hydro-Psychoses*, something (was it his subconscious self?) kept humming in his ears the tune out of the hymn-book: 'Pull for the Shore.'

Numbers XII., XIII. and XIV. of the Minor Studies are (a) 'On Nearly Simultaneous Clicks and Flashes,' (b) 'The Time Required for Recognition' and (c) 'Notes on Mental Standards of Length.' (a) The first research, conducted by Mr. G. M. Whipple, seems to show that the flashes, either by reason of their faintness and the focussing of the attention necessary to take them in, or for some other reason, tend to be perceived before the clicks. Might not, however, the variability of the results of Whipple and those of Exner, Gonesiat and others be due to the visual or auditory type of the subject? Summed up, the results of these experiments (6 subjects) are to show 'a greater attention-claiming quality' of the flash, which makes the interval for recognition shorter for the flash-click than for the click-flash order, and that this holds true for series of pairs. (b) In the second research, by F. W. Colegrove, illustrations from magazines were shown to the subject, some of which he had not seen before, and he reacted, indicating whether he had or had not. The results (from 5 subjects) seem to show that the judgments vary in quickness with the expectation of known (quicker) or unknown (slower) pictures. (c) In the third, by Mr. Colegrove, the mental standards of length were studied by giving 10 subjects a series of fifty circles graduated from $1\frac{1}{2}$ to $4\frac{9}{16}$ inches in diameter and a series of lines the same lengths, and asking them to say how long they were. Three inches was the favorite estimation.

NEW YORK.

WILFRID LAY.

The Dynamogenic Factors in Pacemaking and Competition. NORMAN TRIPLETT. *Am. Jour. Psych.*, Vol. IX., 1898, pp. 507.

In bicycle races the value of a pace may be from twenty to thirty seconds per mile. Mr. Triplett states the theories that have been proposed to account for this wholly or in part. The nature of each of these theories is indicated by their respective titles, namely, suction, shelter, encouragement, brain worry, hypnotic suggestion and automatism. He then advances another theory, stated as follows: "Bodily presence of another rider is a stimulus to the racer in arousing the competitive instinct; another can thus be the means of releasing or freeing nervous energy that he cannot himself release; and, further, the sight of movement, by suggesting a higher rate of speed, is also an inspiration to greater effort." This theory does not exclude the above-mentioned factors in bicycle pacing, but it is supported by laboratory experiments in which most of them were eliminated. The experiments consisted in a flag race. The flags were attached to cord belts that were run by turning a crank like that of an ordinary fishing reel. The races were made alternately with and without a pacemaker or rival, *i. e.*, alternately against time and time plus a rival. Of forty children experimented upon, twenty were stimulated positively; they made greater speed in the presence of the pacemaker. Ten were overstimulated; they lost by the presence of the pacemaker. Ten were stimulated but little. As with wheelmen, the value of a pace was different for different children, but somewhat constant for the same individual in successive trials. Variations for age and sex were small and fluctuating.

In support of the second clause of the theory he cites an experiment from Féré, illustrating this author's theory that the energy of a movement is proportional to the idea of that movement. The third clause of the theory is based upon an experiment in which the speed of counting from one to twenty was increased by 'pacing.'

C. E. SEASHORE.

UNIVERSITY OF IOWA.

L'Invention. Par FR. PAULHAN. *Revue Philosophique.* March 1898.

This contribution to the psychology of invention is characterized by a detailed description of the phenomenology of inventive processes, on the basis of intellectual and emotional experiences of the inventors themselves. The two most obvious generalizations drawn from this wealth of material, including inventions in art, science and technique, are,

first, that all inventive processes are essentially alike in presenting certain well-defined phases, and secondly that these processes are essentially volitional in their type; the main problem which emerges is the question of the relation of the chance associations or suggestions to the dominating idea of the invention.

In all invention there is first of all a tendency of desire, unsatisfied, which imposes upon the mind certain more or less fixed ideas upon which it counts for satisfaction. This is followed by a crisis, akin to volition, in which the dominating idea remains confused and unlogical often until the last moment, and may follow as well as precede the details.

The conditions of this semi-volitional phenomenon are to be found naturally in the sensational and affective sides of consciousness; the former being either general nervous stimulation, as when thought is stimulated by music or walking, or secondly by stimulation through some special artistic sense, either by the same sense in which the invention is conceived or by a law of transposition of the senses, as Paulhan calls it; an invention in one artistic sense may be stimulated by the experiences of another, as when Massenet is stimulated to the composition of oriental airs by the sight of a turban or by the taste of Greek wine, or when Flaubert desires to write a story in *purple*! Color schemes have suggested music, and *vice versa*.

The lack of logical connection between the conditions and results of invention indicates that the connections between the ideas are often largely emotional; and to this color is lent by the foregoing facts, which lead the writer to compare a specialized artistic faculty, which may be thus variously stimulated to the eye nerves whose functioning may be brought about by other than the normal stimulus. As further proof of the volitional nature of invention, it is shown that unsatisfied passions and instincts are often effective causes, Chateaubriand and Rousseau furnishing neat examples.

This tendency to affiliate invention with the volitional rather than the logical side of consciousness—going so far indeed as to consider the difference only one of content—leads naturally in the direction of reducing invention to a continuation of instinctive life, and to the false view which Ribot holds out, that it is capable of a purely nervous explanation. But the author saves himself from a too mechanical point of view—which when pressed must resolve itself into James' chance tipping of the nerve cells—by refusing to call in the element of chance, and by substituting for the fascinating definition of M. Paul Sauriaü, that the element of 'hazard' in invention is 'the conflict of

external casualty with internal finality,' a more comprehensive notion of invention as the resultant of a conflict of different systems of internal finality. This conception M. Paulhan has, unfortunately, not developed further, and the reader will miss likewise a detailed treatment of the social *criteria* of the reception of an invention. A study of these *criteria* from M. Paulhan's volitional standpoint, such as Baldwin has developed from a different point of view, is necessary to the completion of his study. In conclusion we can only call attention to the interesting discussions of the relation of invention to imitation, and to the sources of the subjective sufficiency of an invention.

WILBUR MARSHALL URBAN.

On the Alleged Sensory Functions of the Motor Cortex Cerebri.

E. A. SCHAEFER. Journ. of Physiol., Vol. XXIII., No. 4, Nov., 1898. Pp. 310-314.

This important though brief article or report was read before the Congress of Physiologists at Cambridge, England, in August, 1898. It has especial interest at this time because of the prominence which discussions of the will as bodily action hold in current psychological discussions.

H. Munk had made the assertion, followed by many, that "after total extirpation of the arm- and leg-area [of the cortex of monkeys] tactile sensibility of the opposite extremities is permanently lost; a touch or light pressure is without any effect; neither tactile reflexes nor eye- nor head-movements are produced. The same is the case if the whole arm- or leg-region is removed in monkeys; tactile sensibility is permanently lost in the opposite arm or leg." It was to test the truth of this assertion that this set of thirty experiments was performed on the cortex of monkeys. They were made (1) upon the area connected with the movements of the face (already published); (2) upon the area connected with the movements of the leg; (3) upon the combined area connected with the movements of both arm and leg; and (4) upon the gyrus fornicatus.

These experiments lead Dr. Schaefer to assert boldly that the above cited statement of Munk is "entirely erroneous; that, in fact, complete voluntary motor paralysis of a part may be produced by a cortical lesion without perceptible loss of tactile sensibility" (a fact certainly in accord with frequent clinical experience). "It cannot, therefore, be the case," says the writer. "that the motor paralysis which is produced by a lesion of the Rolandic area is due to a sensory disturbance. And it also follows that tactile sensibility is not localized

in the same part of the cortex from which voluntary motor impulses directly emanate." This does not preclude the possibility that branchlets of the sensory sort of nerves enter the motor areas of the brain; indeed, the tingling often felt upon stimulation of the region suggests that they do so. It seems to the experimenter conclusive that repeatedly excision of the motor area produced no anæsthesia in the part which was thereby paralyzed. We shall await with much interest the outcome of this important and seemingly difficult discussion.

GEORGE V. N. DEARBORN.

COLUMBIA UNIVERSITY.

Recent Views as to the Topical Basis of Mental Disorders. DR. KIRCHHOFF. Trans. by A. W. McCORN. Am. Jour. Insanity, Vol. LV., No. 3, January, 1899. Pp. 481-495.

This is a brief review of the present status of the localization of cerebral functions, with especial psychiatric reference.

The region about the fissure of Rolando, formerly called motor, is now shown to be half-composed of sensory fibres from all parts of the body; hence it is called by Flechsig the somæsthetic area. Quite analogous to this is the visual center ('those cortical regions into which the fibres of the occipito-thalamic radiations of Gratiolet extend'—parts of the cortex adjacent to the calcarine fissure), while it further appears that the retina is directly represented, homologously, in the cortex, as to a less perfect extent is the neural portion of the ear. The auditory area is in the posterior part of the superior temporal gyri and in deeper transverse convolutions. The olfactory area is probably in the gyrus hippocampi, and taste is most likely represented in the uncus near the nucleus amygdalæ. Pain may very probably be represented in the external limb of the lenticular nucleus (the putamen) and in the nucleus caudatus. These centers seem to have trophic functions also, and this relation empirical lessening of pain by improved nutrition corroborates. The unity of the whole nervous system is to be recognized continually. Flechsig's notion of thought-centers in the form of association-centers scattered through the cortex between the sensory regions, but mainly three in number, of which two, the middle and posterior, are probably united into one, the seat of the intellect, Dr. Kirchhoff considers tentatively admissible.

GEORGE V. N. DEARBORN.

Neural Dynamics. W. J. HERDMAN. Journ. of the Am. Med. Assn., Vol. XXXI., No. 21, December 19, 1898. Pp. 1211-1214.

This is another theory of neural dynamics and quite in line with the most natural suppositions concerning this doubtful matter; the article was read at the meeting of the American Medical Association held in Denver in June, 1898.

Dr. Herdman likens neurons to charged electrical condensers. The nutritive processes and states of a neuron are indices of its readiness for action, it having a surface-tension and a corresponding electrical potential. The end-organs of sensory nerves serve as avenues of ingress for forms of motion, which latter causes rearrangement of the cell's molecules and a change in the static electrical condition. On the one hand, every change in a neuron acts as a stimulus on every neighboring neuron, but at the same time, by the principle on which electro-magnetic induction acts, each neuron restrains the action of the others, the balance thus being easily disturbable. "Thus conduction and transference of nerve force are, according to this theory, to be regarded as of the nature of electrostatic phenomena."

GEORGE V. N. DEARBORN.

Ueber die Primitivfibrillen in den Ganglienzellen vom Menschen und anderen Wirbelthieren. ALBRECHT BETHE. Morphologische Arbeiten, VIII. Band, 1 Heft, 1898. Pp. 95-115.

This article by Dr. Bethe, of the University of Strasburg, is one more of the reports of very important histological research into the nature of the ultimate neural unit which are making this basal discussion so animated and seemingly contradictory. He is one with Remak, Max Schultze, Nissl, and Apáthy, besides very many others, in considering the fibrilla, and by no means the cells or the neuron, as the anatomical and physiological unit of the nervous system. By an improvement on Apáthy's method he has been able to demonstrate the fibrillæ in the *vertebrates*, namely, in the frog, dog, rabbit, and in man.

After a brief historical review of the work in this direction to date, and considerations in regard to methods employed, he describes the ultimate fibrillæ in, first, the axis-cylinder, and, second, as it appears in the ganglion-cell. Two plates of drawings, thirteen in number, finely represent what he has seen even, without the elaborate and detailed description of the text. He seems to have examined with

his new methods nearly every sort of nerve-cell found in the species studied, as well as the various sorts of conductive structures.

Dr. Bethe considers it probable that the fibrillæ may conduct impressions both toward and away from the cell; that the protoplasmic cell-projections are not neural, but nutritive; that the connection between the cell, especially its nucleus, and the fibrillæ is very 'loose.'

"The result of this research," says its conductor, "I may give in the very words of Max Schultze, expressed more than twenty-six years ago, but not recognized until to-day: 'Hence such a ganglion-cell, out of which a centrifugal nerve-fiber arises, has meaning as the originating organ of this fiber only in the sense that the fibrillæ out of which the axis-cylinder is composed lead to it by way of the lateral branches of the ganglion-cell, while the fibrillæ, which may be seen extending through the substance of the ganglion-cell, do not arise from the cell, but in themselves only surround it after the manner of the branches of the axis-cylinder and continue on into other lateral branches.'"

GEORGE V. N. DEARBORN.

Neuron Energy and its Psychomotor Manifestations. IRA VAN GIESON and BORIS SIDIS. Archives of Neurology and Psychopathology. Vol. I., No. 1, 1898. Pp. 5-24.

This is the first article in the *State Hospitals Bulletin* under its new title and in its greatly improved dress. The object of the article, announced as only preliminary, is thus stated by its authors: "We intend here to set forth, in a concrete, diagrammatic form a theory that attempts to correlate the various general manifestations of psychomotor life with more or less definite physiological processes depending on the expenditure or restitution of nerve energy."

Many useful and precise definitions occur, and the article is especially rich (about three pages) in seemingly elaborate algebraic formulæ expressing the various sorts of metabolism of the neuron-groups. Psychopathies indicates psychic disaggregation correlative to dissociation within constellations of neurons, the neuron itself remaining uninjured; while Neuropathies is defined as "a group of psychophysical manifestations running parallel to fluctuations of static energy and accompanied by organic changes in the neuron." In mental disease psychopathies may become neuropathies, and the latter may go on to cytoclasis through processes of catalysis and cytolysis.

"The cycles in dynamic energy correspond to the physiological manifestations of the nervous system in the activity and rest of the

individual in normal daily life. Concomitant with the expenditure of dynamic energy of the neurons, the individual passes through the active normal waking state, and hand in hand with the restitution of this expended dynamic energy, he passes through the sleeping state of normal daily life. When, however, in the expenditure of energy, the border line is crossed, dynamic energy is used up and static energy is drawn upon. The border line that separates the normal physiological from the abnormal or pathological psychomotor manifestations is stepped over. * * * * *Catalysis* corresponds to liberation of the upper levels of static energy, and is accompanied by *retraction of aggregates of neurons*, bringing about the phenomena of psychophysiological dissociation. Restitution of the energy expended in the catalytic process is accompanied by *expansion* or *synthesis* of the neurons, which are again able to transmit or receive impulses in the particular aggregate to which they belong. An arrest or halt after the expenditure of energy in these upper static levels, corresponds again to a state of retraction of the neuron or catalysis. * * * * Broadly speaking, psychopathies run parallel to the phenomena of retraction and expansion of aggregates of neurons, while neuropathies are concomitant with actual degeneration of the neuron, especially of its cytolymph. * * * * This one continuous process of liberation of neuron energy may cover the life of a single individual or may extend over the life-history of many generations.

"The continuous descending pathological process may spread out in time and space, may extend over a long duration of time and embrace a great number of individuals. The tide of neuron energy may ebb away gradually, leaving each succeeding generation on a lower stage and deeper level in the continuous process of neuron disaggregation and degeneration, thus giving rise to the different stages and manifestations of *congenital degeneracy*. Many of the so-called degeneracies and the congenital diseases of the nervous system arise, we believe, in this way."

"We may conclude this brief preliminary communication," say these two scientists of the Pathological Institute, "with a few laws relating to the metabolic processes of neuron activity: I. Catalysis stands in direct and synthesis in inverse ratio to the number of disaggregated neuron associations. II. All other conditions remaining the same, the instability of a cell aggregate is proportionate to the number and complexity of its associative functioning groups. III. The stability of a neuron aggregate is proportionate to the frequency and duration of its associative activity. IV. The instability of a neuron

aggregate is proportionate to the frequency and duration of the interruptions in its functioning activity. V. The mass of formed metaplasma granules stands in direct ratio to the intensity of cytolysis and in inverse ratio to the progress of cytothesis."

GEORGE V. DEARBORN.

NEW BOOKS.

De la Méthode dans la Psychologie des Sentiments. F. RAUH. Paris, Alcan. 1899. Pp. 305. Fr. 5.

La Nouvelle Monadologie. CH. RENOUVIER and L. PRAT. Paris, Colin et C^{ie}. 1899. Pp. 546. 12 fr.

Wörterbuch der philosophischen Begriffe und Ausdrücke. R. EISLER. In 8 parts. Parts I., II. Berlin, E. S. Mittler u. Sohn. 1899. Pp vi + 1-96 and 97-192. M. 2 each part.

The Foundations of Zoölogy. W. K. BROOKS. New York and London, Macmillan. 1899. Pp. viii + 339. \$2.50.

The Development of English Thought. SIMON N. PATTEN. New York and London, Macmillan. 1899. Pp. xxvii + 409. \$3.00.

Philosophy of Theism. A. C. FRASER. 2d ed. amended. Edinburgh and London. 1899. Pp. xviii + 338.

Manual of Psychology. G. F. STOUT. Vol. I. London, W. B. Clive. 1898. Pp. xii + 240.

Essay on the Bases of the Mystic Knowledge. E. RÉCÉJAC. Trans. by SARA C. UPTON. New York, Scribners. 1899. Pp. xi + 287. \$2.50.

The Public School Mental Arithmetic. J. A. MCLENNAN. New York, Macmillan. 1899. Pp. x + 138. 25 cents.

Anthropological Investigations on one Thousand White and Colored Children of Both Sexes. ALES HRDLICKA. Illustrated. New York and Albany, Crawford Co. 1899 (?). Pp. 86.

Spinoza, his Life and Philosophy. F. POLLOCK. 2d Ed. London, Duckworth; New York, Macmillans. 1899. Pp. xxiv + 427. \$3.

All students of philosophy will welcome the new edition of Sir Frederick Pollock's standard work—so long out of print. This edition is somewhat reduced in size from the omission of the 'critical and bibliographical matter collected' in the former edition. The appendices to the earlier edition are also omitted 'except the English ver-

sion of Colerus.' Certain later bibliographical indications, on the other hand, are now included.

J. M. B.

Lectures on the Republic of Plato. R. L. NETTLESHIP. Ed. by G. R. BENSON. London and New York, Macmillans. 1898. Pp. vi + 364. \$2.75.

NOTES.

THE 'Teacher's Professional Library' is the title of a series of books announced by The Macmillan Company under the general editorship of Professor Nicholas Murray Butler, of Columbia University. The contributors to this series will be leading teachers and students of education in Europe as well as in the United States. A number of volumes have already been arranged for.

PROFESSOR EDWARD BRADFORD TITCHENER is preparing for publication early in the fall 'A Laboratory Manual of Experimental Psychology,' which will be published by The Macmillan Company. The work will be in two volumes and will detail an elementary course of laboratory work. The first volume will deal with qualitative analysis, the second with the exact measurement of mental processes. Each volume will be published in a student's and a teacher's edition, the former giving instructions as regards the conduct of experiments, control of introspection, etc., and the latter furnishing references, cognate questions and exercises, and standard results. (Publisher's note.)

WE notice in the *Journal of Mental Science*, which is much improved in form and appearance, the beginning of an 'Index Medico-Psychologicus,' prepared by Dr. J. Turner. The first part goes from A to I, for the year 1893-4. The arrangement is alphabetical simply.

DR. DODGE has been advanced to an associate professorship of philosophy at Wesleyan University.

MESSRS. MAYER AND MÜLLER, of Berlin, are publishing in three volumes the mathematical correspondence of Gottfried Wilhelm Leibnitz, under the editorship of C. J. Gerhardt.

DR. BENJAMIN RAND, of the department of philosophy, will publish in April a work entitled 'The Life, Letters and Philosophical Regimen of the Third Earl of Shaftesbury.'

EDWARD THORNDIKE, Ph.D. (Columbia), instructor in education in Western Reserve University, has been appointed instructor in genetic psychology in Teachers College, Columbia University.

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